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THE JOURNAL OF
ABNORMAL PSYCHOLOGY
AND
SOCIAL PSYCHOLOGY

VOL. XVIII JANUARY–MARCH, 1924 NUMBER 4

ORIGINAL ARTICLES

THE PSYCHOLOGIST IN THE PSYCHOPATHIC HOSPITAL

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AMONG the newer developments of psychology has been the measurement of mental abilities and the application of laboratory methods to the determination of special abilities and disabilities. That the tests and scales and technique thus developed could be of use in psychopathic cases has been suggested several times, and attempts have been made to study such cases by means of these methods and to improve upon them for use in psychopathic hospitals. There are many important questions arising in the hospital that can only be settled by careful psychological study of the individual case. Our present methods are perhaps crude and vague, and not usable in some cases, but in others they are not only of service but point the way to a still further development of psychological method.

During the past two years a total of 200 cases in St. Elizabeth's Hospital have been studied, with a view to determining how far our present methods are available in psychopathic hospital cases. Some of the results of this study are here briefly set forth.

The methods used were various tests calculated to display different mental functions, such as memory, apperception, associative ability, ability to judge and compare, to interpret pictured or verbal situations, to synthesize and generalize, and so on. The most intensive work was done with the Stanford Revision of the Binet scale, since it is the best standardized and therefore affords the best opportunity for comparison of individual reaction with norms, or the usual reactions.

Extensive use was also made of the Kent-Rosanoff Association test, while recently a series of Porteous Maze tests has been begun.

Nearly every form of mental abnormality is to be found among our cases, seniles and general paretics alone being excluded. A considerable group of the men were confined in Howard Hall (the Federal Prison) and classed as criminal insane.

Considerable difference is to be found between the male and female cases, which arises from the fact that St. Elizabeth's is a Government hospital, admitting patients from the army and navy, and therefore obtaining many men in their first breakdown and soon after the onset of the psychosis; while the female cases are from the District of Columbia and pass through an observation hospital from which the milder cases are dismissed, so that as a group the women are older than the men and a higher percentage of chronic cases is to be found among them. Again, because the hospital has no observation ward, the drug and alcoholic cases and the acute psychotic episodes occurring in defective and psychopathic individuals are far less frequent than in the city hospitals which maintain psychopathic wards.

In nearly all the female cases the psychological examination was given soon after admission or as soon as the subsidence of the acute stage would permit; in a few, examination was repeated at intervals during recovery. The male cases were more highly selected, as examination was requested for some special reason, such as suspected mental defect or difficulty in diagnosis.

It may be questioned as to how far it is possible to apply psychological tests to cases such as described above, and how far the results can be considered trustworthy. As regards the first point, we may reply that in every case save two it was possible to obtain the subject's coöperation. Considerable tact and maneuvering was necessary, and often it was not possible to observe time limits and follow a strict laboratory procedure. But the problem was not to obtain a rating or mental age, but to discover how much of a given mental ability was present and what peculiarities of function it showed, as well as the reasons for the latter. To this end not only was it necessary to adapt the method of presentation to the case in hand, but account had to be taken of the qualities of attention and the tendency to distraction, whether endogenous or exogenous, the presence or absence of hallucinations or delusions, the rapidity of association, and the presence of disturbing complexes.

One of the earliest problems to emerge was the differentiation of original mental defect from the deteriorated state resulting from mental disease. A man arrives in the hospital in the acute stage of a psychosis, coming from a ship or an army post, or from a dis-

tant part of the country, and no trustworthy history can be obtained. Later the acute state subsides, and the patient shows mental traits that are commonly diagnosed defective. From conversation with him and from the usual mental and psychiatric examinations he appears feebleminded. The following is such a case:

Chas. B—, age twenty-nine, United States prisoner, transferred from Leavenworth, where he was serving a sentence for robbing a postoffice in Montana. On admission he talked in a slow manner and apparently had difficulty in comprehension of his surroundings. No definite hallucinations or delusions were elicited, but there were some vague references to ideas of persecution. He was correctly oriented in all fields; memory for events and dates appeared fair, though when history was secured later from his sister it was seen that his account of his past life was inaccurate in many points. The report from Leavenworth noted that he had appeared very temperamental, had fits of melancholy, and showed suicidal tendencies. When he was presented at staff conference, before the history of his early life had been received, his appearance and manner were that of a mental defective. The story of his crimes fitted in with this conception of his mentality. They were poorly planned, and executed in a bungling fashion, so that he was easily apprehended. It was the consensus of opinion that he was of very inferior mentality, if not actually a congenital defective. The psychological examination told a different story. The sum of his mental abilities was indeed but nine years, eleven months, but there was unmistakable evidence that it had once been considerably higher. There were remnants of old knowledge and abilities which no longer functioned, except for a moment under special stimulation. He showed great mental inertia, would not put forth the effort necessary to complete a task even when he had grasped the situation and attacked it properly. He thought very slowly, in a slip-shod fashion, and anything requiring sustained attention was beyond him. He was vaguely troubled by this characteristic, realizing that he was making failures and saying that he used to know these things but it had been so long since he had had any use for them. On the basis of the psychological picture the examiner felt that he was not a mental defective, but probably one of those cases which, with or without a definite mental episode, begin to deteriorate during adolescence and spend the rest of their lives on a lower mental plane—the material from which so many of our tramps, hobos, prostitutes, and petty criminals are made. The history obtained later confirmed this view. Charles had been a bright child, learned easily in school, and left at sixteen to go to

work. He did well for three years, then became discontented, and gradually a change of personality became evident. He was not as sociable as before, grew very quiet, never "looked on the bright side of things." He seemed restless, trying first one job and then another, wandering about the country. While in this condition he robbed a postoffice, and thus finding himself in possession of money, continued to enter postoffices until he was discovered and arrested. It thus seems highly probable that the prisoner was irresponsible when the crime for which he is serving sentence was committed. A study of our material reveals many such cases.

Case No. 2. Samuel S—; age eighteen; referred because be was thought to be defective, gave indication not only of not being an original defective, but of having been of quite superior intelligence.

Case No. 3. Julius N—, who was thought to be either malingering or defective, was examined twice while in an acute confusional state, and later when he had recovered from the acute attack. Again the apparent defect proved to be the residue of a psychosis, as it was quite evident that Julius had once been of at least average intelligence.

Still another interesting case: No. 4. Frank F—, who in preliminary conference appeared so dull and lacking in comprehension, with no psychotic features apparent, that it was thought more than likely he was a mental defective. Referred for psychological examination to discover if he were likely to profit from psychotherapy. Examination showed inability to concentrate attention and considerable psychomoter retardation; memory was unreliable, sometimes functioning well and again poorly; comprehension was good when he was given time. On the basis of his reactions the examiner surmised that he was phantasying a good deal, which he admitted. His original endowment was at least average, and probably above. He proved a good subject for psychotherapy.

But how does the mental picture of the defective differ from that of the patient who has formerly been of higher intelligence? Too often our conception of the mental defective is a social one and not psychological at all. A man is defective because he cannot "compete with his normal fellows" or cannot "adjust on the social level in which he was born." But there are many reasons for lack of adjustment and failure to hold one's own, and if defect means anything in psychological terms it must mean a cessation of development at a point so far below the average that the individual can never be expected to grasp the reality or use the intellectual tools of the ordinary adult world. That such arrest of

development is a fact we all know. The person in whom such arrest has occurred on the eight-year level may continue to acquire knowledge for a good many years, but it is knowledge *on his level.* Given the best chance in the world and his vocabulary, his comprehension, and reasoning ability, his power of interpretation and ability to form new associations will remain on the eight-year level. The picture, then, in simple defect (by which we mean defect uncomplicated by mental disease or functional disturbances) is a fairly even one. The different abilities are functioning on practically the same plane, development has ceased at a certain level, and there, psychologically speaking, the individual remains. However, may not a psychosis leave an individual in the same state?

Case No. 5. Dora L—, age twenty-five on admission, eighteen months ago, passed through a prolonged catatonic excitement, finally settling down into a dull and stupid state, where she sat idly all day in a corner of the ward. She had been a young woman of fair intelligence, and entrance examinations were well performed. Twelve months after admission she was able to coöperate in psychometric examinations; her rating on the Stanford-Binet was eight years, ten months, with a basal year of six and no credits beyond nine except for the comprehension questions in year ten. She was very apathetic and her attention was hard to obtain. Memory was functioning on the ten-year level, but the other abilities were consistently below. Power of discrimination was so poor that she could not differentiate a fly from a butterfly nor wood from glass—a test standardized at the seventh year. At first sight the picture certainly resembles that of a defective. However, close study rewarded us with some interesting details, suggestive of an original higher level. When she is stimulated and effort insisted upon she *can* do much better than she actually does do. Like Case No. 1, she has not energy enough to carry her through a task even when correctly begun. She begins one of the arithmetic problems correctly, but falls to dreaming again, and says, "Seems like I can't do it." The same is true of some of the twelve-year tests. She uses an excellent vocabulary, though she cannot define the words she uses. Her drawings are well begun, then become slipshod and lose all resemblance to the original. Her general level of efficiency is greatly lowered, but the evidence of a former higher level is unmistakable, slight though it is. Moreover, we are not justified in calling this level a state of deterioration, as she is still actively psychotic, hearing voices and experiencing an occasional visual hallucination. We must think of it as a regression, and

hold out hope of ultimate improvement, even while we deal with the patient on the childish level where she at present belongs.

It is sometimes said that the influence of emotional factors upon the responses in an intelligence test may make the individual appear defective. In *Mental Hygiene* for January, 1922, Jewett and Blanchard have reported several such cases examined in Bellevue Hospital. For some reason such cases do not appear among our material—perhaps because we were eternally on the lookout for emotional factors.

Some of the test situations are well calculated to display them; such are the free associations, the reading, and especially the report, all the vocabulary tests, the absurdities and fables, and the problems of fact. Again, there is a considerable difference between the emotional attitude of a defective, however dull and stupid he may be, and the flattening of a precox, the retardation of depression, or the blocking of a psychasthenic state.

So far we have been speaking mainly of "mental deterioration," meaning thereby a permanent state of lowered intellectual efficiency consequent upon a psychosis. How far does this involve emotional deterioration, and is there any danger of mistaking the one for the other?

Case No. 6. Lydia M—, age twenty-five upon admission; married. Diagnosis: Hebephrenic dementia precox. She had been ill for four years, having the first acute upset following an attack of influenza, though she had been very nervous for some time previous to this. She had been in and out of sanitariums, improving for a time, then relapsing again. Upon admission she was alert and talkative, willing to discuss her difficulties, and gave the impression of being fairly intelligent. Orientation was good, and auditory hallucinations were remembered but their existence denied at the time of admission. Her response to the routine intelligence tests was poor. A Stanford-Binet was given three weeks after admission. She was now very apathetic, indifferent, sat hunched up in her chair and staring out of the window with dreamy eyes. She had to be aroused, stimulated, shocked into attention, and relapsed into her dreamy state after a moment's attempt at response. The test was a very uneven performance, credits ranging from the fifth through the twelfth year. Memory (immediate reproduction) was poor in all forms tested, evidently because of inattention, as she was able to hold in mind five digits and repeat them backward when special effort was made to stimulate her. She was disoriented in time, and comprehension and reasoning were not functioning above the nine-year level. She

remained in the hospital ten months. Her conduct was variable, sometimes being marked by excitement. Sometimes she was pleasant and agreeable, again noisy and profane. Most of the time she showed great emotional apathy and indifference. After about eight months she seemed considerably improved; showed some insight; said she had been "out of her head" and heard voices, but they had ceased to trouble her. She was once more alert and talkative, oriented, and began asking to go home. Her emotional attitude was nearly normal. In this state she was given another psychological interview, with the surprising result that her mental rating had dropped nearly a year. She was alert and reacted quickly, with only a few lapses of attention; she talked at great length, using good English, but talking around and about a subject, and getting more confused and farther away the longer she talked. She was not able to comprehend several things which she had accomplished before, but made a great effort to conceal her lack under a multitude of words. Here, certainly, mental and emotional deterioration were not running parallel, and a great improvement in general attitude and emotional tone was accompanied by a still further lowering of mental efficiency. This patient did not hold all that she had gained, but it raises the question of other cases in whom a return to a more or less normal attitude may mask a degree of intellectual deterioration that in itself will prevent a social adjustment. There are patients who retain a verbal knowledge of situations they have once understood and been able to handle, so that they can discuss the subject with apparent intelligence, but whose ability to actually do the thing they can talk about is nil. When put up against the actual situation their verbal knowledge cannot be translated into motor terms. This peculiarity is not the exclusive property of people who have passed through a psychosis, and the reasons for it are a fascinating subject to the psychologist. The Freudians have so far been the only ones to shed much light upon it, but to the writer's knowledge no investigation of this phenomenon in the insane has been attempted.

More than once it has happened that the psychotherapist has spent much time upon a promising patient who has perhaps passed through a psychotic episode and is now clearing up. He is seemingly alert and in touch with his environment, and his general appearance and manner would indicate that he is a good subject for psychotherapy. But no headway can be made; the patient recounts his difficulties and apparently accepts explanations, but gains no insight, and analysis goes round and round in a circle,

getting nowhere. Psychological examination frequently reveals a situation similar to that in Case No. 6—his intellectual abilities are functioning on a level so low that real comprehension of his difficulties cannot be expected of him. This type of patient is also the source of much trouble to the Out-Patient Department. One job after another is found for him; there are difficulties here, there, and everywhere. The social worker keeps on, blaming his failures upon the residuals of his psychosis, but how great those residuals are she often fails to realize.

Case No. 8. Minnie D— is a case in point. Minnie was a colored woman of twenty-eight or thirty, who passed through a severe precox episode and apparently recovered. She was a little flattened emotionally, but on the whole seemed quite well, and was able to talk with fair intelligence about her experience and her plans for the future. But Minnie proved to be a severe burden to the social worker. She could do beautiful laundry work, but could never make a financial adjustment with her customers, and was always having to be helped out of difficulties. In despair the social worker appealed to the psychologist. Minnie's abilities proved to be mostly verbal. Her financial difficulties with her customers were explained by the fact that she could not make change, even for a quarter. She could add dollars, but any other piece of money threw her into confusion. In general her abilities were so poor that constant supervision of her activities seemed imperative. Yet she was no longer psychotic and therefore not entitled to hospital residence.

It is difficult to demonstrate the reverse of this picture, *i.e.*, great emotional dilapidation accompanied by unimpaired intellectual functioning—comparatively speaking, of course. But occasionally a case like the following presents itself:

Case No. 7. Louis T—, age twenty-five, has been a resident of this and other hospitals for three years, and variously diagnosed catatonic dementia precox, simple precox, psychoneurotic, psychopathic personality. In this hospital he was a great problem, appearing utterly ambitionless, unable to stick to anything or to make any sustained effort toward improvement. Two psychotherapists did their best for him, but after fifteen months he appeared such a "mental, emotional, and volitional indigent" that psychotherapy was dropped. A psychological examination was difficult to obtain, because of his emotional apathy, but the examiner persevered, using every means of stimulation at her command, and was rewarded by the discovery of excellent abilities, still capable of quick and accurate function when their owner had

sufficient incentive to employ them. Far from being a "mental indigent," he was unusually richly endowed, and there was no evidence of impairment or deterioration. The findings were in striking contrast to his complete inability to use his intelligence in meeting ordinary life situations.

Another question of great interest to the writer has been the psychological differentiation of the psychopath and the psychoneurotic. A great deal of attention has been paid to emotional make-up and type of personality in the constitutional psychopath, the study of the psychogenic bases for his conduct have added to our understanding of him, and, thanks to Freud and his school, we know a good deal about the psychoneurotic. But the two conditions are sometimes very hard to differentiate; much valuable time is spent by the psychotherapist upon the psychopath, who ill repays him for his pains. Is there any distinctive psychological picture in one or the other case?

Case No. 9. Wm. L—, aged nineteen; sent to the hospital from the Naval Hospital in Washington. He is a good-looking lad, with the face and general attitude of a much younger boy. He is pleasant most of the time, smiling and laughing quickly, but is impatient, restless, and difficult to hold to a stated task. He gives a history of attacks of an epileptoid nature, and has received a good deal of study because of the obscurity of the diagnosis. Letters from his father, a very illiterate man, inform us that he was always headstrong, could never tell the truth, went to school irregularly, and learned little. He could never get along with his family or relatives, began drinking very young, was in jail a number of times, could never make an industrial adjustment. In short, his history is typically that of the psychopath. Psychologically this boy's abilities are very irregular—some of them good, even excellent, others so far below the average as to rate him feebleminded. In anything involving arithmetical reasoning he does well; figures have a meaning for him; although when called upon to reason in a situation other than mathematical he does very poorly indeed. On the Stanford-Binet test he rates eleven years, three months, with credits scattered from the eighth through the sixteenth year. This irregularity of ability seems to the writer very typical of the constitutional psychopath. Apparently the life-energy has not been sufficient to carry him to full maturity, so that though some abilities are normally developed and able to function at adult level, others remain fixed at lower levels, and thus prevent that integration of the personality which is essential for normal adjustment. Analysis may enable us to discover why the particular fixations

have taken place, but scarcely serves to round out or resynthesize the individual so that he becomes the possessor of an adequate personality, since the primary difficulty is energy lack, and one ability must gain at the expense of another. The psychopath who succeeds in overcoming one fault "breaks out" into another. One ability may be highly trained, but simultaneously others decline. He has only just so much energy to spare for his life task. The psychoneurotic, on the other hand, shows no such rigidity in the intellectual sphere. His abilities are much more evenly developed, though their function may be impaired or obscured, more or less permanently. But underneath the inhibitions and repressions the ability is there, and capable of function when the locked-up energy is released. How to discover this in a psychological examination becomes the problem. It frequently happens that in such an examination the psychoneurotic shows up well. It is quite evident that there is no reason for his difficulties in his intellectual endowment. In hospital cases, however, whose lack of adjustment has become so great as to result in their removal from the community, either voluntary or otherwise, this is almost never true. Some of the abilities are pretty sure to be functioning abnormally. If they were not, the individual would still be able to hold his place in society.

Case No. 18. Thea W—, age thirty-three, an army nurse, had had a severe illness two years before admission, and had been going the round of Veterans' Bureau hospitals ever since. She came with a diagnosis of dementia precox, based largely upon the fact that she had declared herself to be another person while in the hospital from whence she was transferred. On admission, however, she was perfectly clear, oriented, and told a plausible story apparently with complete insight. Hospital residence of eleven months and a searching analysis failed to disclose anything deeper than hysteria. In the psychological examination given eight days after admission it was noted that, though her abilities were irregular, it was not because of primary lack of endowment. Her speech was distinguished by circumlocution and prolixity, and reaction time was much delayed. It seemed impossible for her to come to the point or to deal with situations that required a choice of reactions. And yet this was true of only certain situations; others, inherently more difficult, were easily met. She complained of being "stupid" and was greatly worried by this stupidity and by the retardation, of which she was fully aware, but unable to control. Her general attitude was very different from that of the psychopath, who is almost invariably satisfied with his own per-

formances, and projects his difficulties upon his environment. This irregularity of an ability *within its own field,* so that, though it may be functioning abnormally, it is definitely present, is conspicuously different from the complete lack of an ability which we observe in the psychopath. This conclusion is only tentative and needs a wider range of tests as well as a greater number of cases to verify it, but it seems worth while to state it as definitely indicated by our material.

Space forbids further discussion of the problems suggested by our material. The paranoids, who as a group rate the highest of all our cases, form a most interesting study in themselves, as do the epileptics. And there are a few cases who have been followed through the entire course of their psychosis which will be reported later. Nor can we here discuss the association tests, except to say that in general our findings confirm those of Kent and Rosanoff's original investigation of association in the insane.

The psychologist, even though greatly handicapped by lack of instruments of precision in mental measurement, still is able to be of considerable assistance in the psychopathic hospital. In order, however, to enable him to give the best service he must be a person trained in psychiatry and with actual experience in the different forms of mental disorder. It is possible, with the means now at command, to distinguish between original mental defect or feeblemindedness and the deteriorated state resulting from mental disease. This is of considerable value in preventing the expenditure of effort in the attempt to raise a subnormal individual to a level he never possessed. It is also possible to recognize in an individual who has recovered from a psychosis with a fairly normal emotional attitude, alert and apparently in touch with his environment, a degree of intellectual deterioration which in itself will prevent his social adjustment at a level at all approximating his former one. We all know that this state of affairs exists, but in the individual case it is usually discovered by the trial and error method. Where the question is one of vocational rehabilitation the psychological examination should include aptitude tests and tests for special abilities. These we have not yet been able to use in this hospital. In addition the psychologist is in touch with fascinating problems. How far "mental" and "emotional" deterioration run parallel, and whether we are ever justified in speaking of "deterioration" short of the autopsy table; the psychological distinction between the psychopath and the psychoneurotic; the intellectual make-up of the paranoiac—these and many other problems suggest themselves.

We are greatly in need of more and better tests, tests standardized upon normal adults. The ones we have cannot be used according to rule, but must be adapted to our purpose. This, however, is not of great importance, since we are not concerned in rating our patients or classifying them according to "mental age" or I.Q., but discovering how much of a given ability is present and capable of function. The "how much" of course can only be determined by reference to some norm and until better ones are established we are using those of the army.

A further study upon the higher thought processes in the insane is in progress and will be published later in the year.

THE INFLUENCE OF ALCOHOL (Part II)

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EDITORIAL NOTE: In the first installment of this article, which appeared in the October-December number, Professor Hollingworth described the methods and part of the results of his investigation of the influence of alcohol in beer form upon the functions used in tests of steadiness, coördination, tapping, color naming, mental calculation, naming opposites, and substitution. The present installment, which concludes the investigation, presents further experimental data and interprets the significance of individual differences in susceptibility to the effects of alcohol.

VII

THE SPECIAL DAY

On the next to the last day, as described, a special method of dosage was adopted. On the two preceding days, all subjects had taken five and three bottles of beer. After the third test of this forenoon each man drank one bottle of Standard beer. The fourth round of tests was then followed by a second bottle, the fifth round of tests by a third bottle, and after the sixth round of tests the usual drinking period of one hour came. During this period each man, having already consumed three bottles at half hour periods, drank as much as he could during the hour. The individual totals ranged from six to nine bottles, more, it is safe to say, than any member of the squad would have taken during a similar period except under the urge of experimental conditions and for valuable consideration.

The purposes back of this special day were several. In the first place it was desired to know after how small a dose quantitative changes would be found. Further, it was desired to compare the various types of work from this point of view. It was also desired to learn how much alcohol could actually be consumed in beer drinking during an experimental day, and what might be the effects of such maximal amounts, on the quantitative records, on the subjective feelings and attitudes of the subjects, on their general organic reactions, and on their conduct and bearing as reported by the observers. Only the quantitative records are here considered.

This set of alcohol records is less reliable than the others for various reasons. The doses were not equal with all subjects; there is only one day's record; these maximum doses produced in all cases marked organic revulsions in the form of vomiting, dizziness, and general illness, which complicate the picture. When the amount of this standard beer taken into the system exceeds the

TABLE 1

Squad Averages, Special Day

First figure gives score. Second gives score as per cent of pre-dose average.

Trials	Average 1–4	1	2	3	4	5	6	7	8	9	10	11
Pulse	70.5	76.9 109	70.3 100	66.8 95	68.1 97	70.6 100	67.3 96	80.1 113	80.3 114	80.2 113	76.1 108	76.1 108
Steadiness	10	13 130	9 90	9 90	10 100	15 150	14 140	47 470	32 320	47 470	32 320	29 290
Tapping	488	453 93	494 101	501 103	503 103	499 102	476 98	388 80	403 83	374 77	428 88	450 92
Coordination	151	150 99	152 101	153 101	151 100	148 98	147 98	114 76	124 82	125 83	125 83	127 84
Color Naming	48.9	49.9 102	49.5 101	45.1 90	51.1 102	54.8 109	51.6 103	63.4 127	64.1 128	64.0 128	53.4 107	50.8 102
Substitution	106.4	109 102	107 100	113 106	96 90	96 90	101 95	88 83	93 87	100 94	93 87	95 89
Opposites	39.8	39.1 98	42.6 105	38.2 96	39.3 99	40.5 102	51.4 129	60.6 152	54.9 138	53.0 133	52.5 132	41.7 105
Adding	60.5	62.6 103	60.6 100	61.8 102	57.0 94	62.3 103	73.8 122	76.5 126	73.3 121	79.0 130	68.7 114	64.7 107
Memory	7.1	7.1 100	6.7 96	8.2 117	6.3 90	5.5 79	4.2 60	1.6 23	2.3 33		2.0 29	
Dose				1st bottle	2nd bottle	3rd bottle	Long period, 6th to 9th bottles					

amount required to produce the changes shown by the five and six bottle records, the organism rebels against it and sickness ensues, characterized by nausea, headache, vomiting, dizziness, a bloated feeling, an uncomfortably copious flow of urine and a general repugnance toward the taste of the liquid. The amounts of beer taken on the special day were about twice as great as the best of the beer drinkers among the subjects felt to be a comfortable amount for the period of time involved. The maximal amounts were consumed only because the compensation of the subjects for the whole experimental period depended in part on their persisting to the end of the investigation, and the last bottles on the special day were uniformly drunk under protest and with loathing.

The effects produced at the end of the special day were thus achieved only by producing wretched illness and by the prescription of the doses in a scientific fashion, on the two preceding days and on the day in question, so as to produce the maximum possible influence, taking advantage of all possible cumulative effect by gradually increasing the amount of alcohol in the system up to the point at which the effects of the beer first taken had definitely begun to disappear.

The individual reactions can be observed by referring to the original tables. The general result is shown in the following table. Here the first four trials are averaged to constitute a unit, into terms of which all trials are transformed, as shown in the second line of each test. The first line gives the actual average record of the six subjects at each trial. Considering the second line, in each test, the records may be considered as definitely affected when they begin to depart markedly from 100%, the average of the first four trials. Using this rough standard, the following conclusions seem warranted.

The first two bottles, as shown by the records in trial six, made just after taking the third bottle and before the alcohol in this third bottle has begun to act, do not definitely affect Pulse, Steadiness, Tapping, Coördination, nor Color Naming. But all these tests show material loss at the seventh trial, after the mid-day drinking period. It would seem from these averages that Opposites, Adding, Substitution, and Memory for Paired Associates * begin to show

* The test of Memory by the method of paired word associates was introduced on the last three days of the investigation. One of these days was the dinner day. On this day the average afternoon work was 18 per cent inferior to that of the forenoon. On the three bottle beer day the afternoon loss was 21 per cent, and on the Special Beer day here considered the afternoon average loss was 60 per cent. No records on Blank or Control days are available. This test is reported more fully in a later section.

definite impairment by the sixth trial, after two and not more than three bottles of beer have begun their effect.

It seems entirely justified to say that the former group represent simpler and more automatic processes, whereas the latter group might be characterized as more distinctively intellectual or mental operations. Color naming and Substitution probably represent the dividing line between two such groups.

We must conclude therefore either that the more strictly mental or intellectual processes begin to show their impairment at an earlier stage, or else that they are influenced by smaller amounts of alcohol.

VIII

INFLUENCE OF A MEAL

In so far as the experimental meal influences the course of performance in these processes, it should be disclosed by comparing the records on the Dinner day with those of Blank days. It is to be noted that on the dinner day only four forenoon and four afternoon trials were given. As indicated in the section on "Schedule of Doses", the meal taken on this day was a conventional dinner, accompanied by a "small coffee" and a cigar or cigarettes. The chemical agents in tobacco and in coffee, even in these small doses, are known to produce measurable effects on test performance. But in the present connection these influences are negligible for at least three reasons. In the first place tobacco and coffee produce opposite effects on most of the tests here employed. In the second place it is well known that the effect of food in the stomach is to reduce and retard, and, in the case of small doses, quite to nullify observable drug effects. But the third and main reason is that the purpose of this part of the experiment was not that of determining the effect of food alone, but instead that of measuring the effect of a conventional meal, such as that here described.

The pulse records show a very decided rise after the dinner, contrasting markedly with the tendency to retardation shown on Blank days. This acceleration is characteristic of all six individuals, the average increase in rate being over fifteen beats per minute.

Characteristic also of all the individuals except II and perhaps IV is the unsteadiness shown during the afternoon following the meal. Instead of the normal tendency toward increased steadiness as the day goes on, there is increased unsteadiness instead.

In Tapping the tendency on Blank days is toward inferior afternoon performance. After the meal there is instead an increased score in the average records. This is shown by all the individuals except IV and VI.

Coördination records on Blank days show definite improvement in the afternoon. After the meal this gain is not present, so that there is a relative loss in proficiency.

Color naming suffers marked afternoon impairment on Blank days. After the meal there is no evidence of such loss, but instead a relative gain that is considerable. This situation is characteristic of all the individuals except III.

In Opposites the usual loss of Blank afternoons is not present after the meal, and there is with most of the individuals, and on the average a slight gain instead. The relative gain is thus very evident.

Adding behaves in quite the same way as do Color Naming and Opposites. Instead of the afternoon loss characteristic of blank days there is an appreciable relative improvement.

A rough statement of the effect of the meal may be secured by comparing afternoon and forenoon performance on the dinner day with the same measures on Blank days. Such a comparison is given in the following table. In the first column is given, for each

TABLE 2

Process	Blank Days	Dinner Days	True Dinner Effect
Pulse	—.07	+.17	+.24
Steadiness	+.11	—.50	—.61
Tapping	—.02	+.06	+.08
Coördination	+.03	+.01	—.02
Color Naming	—.06	+.02	+.08
Opposites	—.02	+.03	+.05
Adding	—.04	+.01	+.05
Substitution	—.01	+.02	+.03

process, the average per cent gain or loss on afternoons of Blank days. The second column gives the same figures for Dinner day, and the third column gives the difference between the two days. The basis of comparison is in all cases the average forenoon performance. In all cases + indicates gain and — indicates loss in efficiency.

The absolute difference between afternoon and forenoon work on Dinner day is small except in Pulse, Steadiness and Tapping. But when compared with the normal tendency of Blank days in which all details were the same except the amount of food taken, the differences, as shown in the third column, are very considerable.

The changes in the tests are all in the direction of improved performance, except in Steadiness and Coördination. The loss in Steadiness is very striking and is equal to that produced by small doses of alcohol.

The Pulse is quickened notably by the dinner, the acceleration being considerably greater than that produced even by the extreme alcohol doses of the Special Beer Day.

In the remaining tests the *quantitative* effect of the heavy meal is quite comparable in amount to the effects produced by three or four bottles of beer, and in Tapping and Color Naming even to the effect of five and six bottles. The *qualitative* results, however, as shown by the direction of the change in performance, are opposite to that of the beer in all the processes except Pulse, Steadiness, and Coördination. In these three tests the Dinner and the Beer produce the same type of changes, namely increased pulse rate, and impaired steadiness and coördination. In the remaining tests, Tapping, Color Naming, Opposites, Adding and Substitution, the effect of the dinner is either relative or absolute gain as compared with Blank days, whereas the effect of all beer doses is impairment as compared either with Blank or with Control days.

IX

ALCOHOL AND CAFFEINE

Inasmuch as six of the tests here employed have also been used in an investigation of caffeine (H. L. Hollingworth—The Influence of Caffeine, Arch. Psychol. 22, 1912) there is a certain interest in the comparison of the two sets of results. In fact the significance of the changes produced by one drug can be more readily evaluated when they are compared with the effects of some other agent with which we are more or less familiar.

The average large cup of coffee is found to contain about 2.5 grains of caffeine, and the average glass of cold tea about 2 grains. In the caffeine investigation the doses were of 1, 2, 3, 4, or 6 grains. Since the effects of the one and two grain doses were slight, comparison will be here made of the three to four grain and the six grain caffeine doses, on the one hand with the effects of small and large alcohol doses as described in the foregoing account. The records of the Special Beer day (see p. —) are also given, although the main interest is in the comparison of moderate amounts of beer with moderate amounts of caffeine.

The caffeine records of Squad IV are chosen for comparison

with those of the beer squad because this is the caffeine squad that took the dose on an empty stomach and relatively late in the day. This is more nearly like the condition of the alcohol dosage than was that of any other caffeine squad. Further, Squad IV was the only one containing as many as five subjects.

In all the data given here the average performance before the dose, is taken as the basic unit of all comparisons and the figures in each case mean per cent of forenoon record. The plus sign always indicates improved work and the minus indicates impairment.

The alcohol averages as given in this table are more reliable than are the caffeine averages. In the former case there are six trials before and the same number after the dose. In the case of the caffeine data, there were three trials before the dose and two after it. Both cases, however, cover a period of about three hours after the dose. The individual caffeine records and their deviations from the averages may be seen by referring to the caffeine monograph.

TABLE 3

EACH COLUMN EXCEPT THE FIRST TWO GIVES THE NET EFFECT (IN EXCESS OF CONTROL EFFECT) OF THE VARIOUS CONDITIONS INDICATED AT THE COLUMN HEADS.

	Blank	Control	Dinner	Bottles			Grains	
				3–4 Beer	5–6 Beer	6–9 Beer	3–4 Caffein	6 Caffein
Pulse	—7	0	+24	+8	+10	+19		
Steadiness	+11	—21	—61	—68	—241	—370	—2.0	—583.0
Tapping	—2	0	+8	—7	—13	—14	+3.1	+3.8
Coordination	+3	—2	—2	—6	—10	—20	+0.3	—4.0
Color Naming	—6	—3	+8	—2	—7	—12	+7.0	+4.4
Opposites	—2	—7	+5	—5	—12	—23	+5.1	+6.2
Adding	—4	0	+5	—10	—15	—16	+1.8	+2.5
Substitution	—1	—5	+3	—4	—9	—6	...	...
Memory	...	...	—18	—21	...	—60	...	...

The table contains eight columns of records. In column one is given for each test the characteristic tendency on Blank days, for the beer experiments. Thus there is on the average a decrease in pulse rate of 7 per cent, an increased steadiness of 11 per cent, etc.

Data for the control day and the dinner day are also included here for purposes of comparison. Pulse records and Substitution records are also included, although there are no caffeine records

for them. Data from the three days on which the Memory Test was used are also given (although in this case there are neither Blank, Control, nor Caffeine records), in order to make this table a complete summary of the results of the experiment.

The second column contains the Control day averages, the net change on Control days as compared with Blank days. Thus on Control days Steadiness showed a decline of 21 per cent below the record for Blank days, the absolute loss thus being 10 per cent of the forenoon record.

The dinner column gives the net effect of the dinner as compared with the blank days as a control—not therefore the gross change noted after the dinner but the amount of change to be directly attributed to the meal, since not present on blank days. The next three columns give the true beer effects for small, large, and special doses. This again is not the gross effect observed on beer days, but this gross effect minus the effect present on control days. The figures thus represent the effect to be attributed directly to the alcohol.

In the caffein columns are given the net effects of the two amounts of caffein, a result secured by subtracting from the gross effects the afternoon changes observed on control (sugar of milk) days. Color Naming records, however, were not used in this section of the caffein investigation (Experiment B), hence the records from Experiment C on twelve subjects are here used. The table gives as the caffein effect on this test the difference between the absolute effect on caffein days and the average of the blank and control days. (Note: The original tables from which the caffein data here used are taken are to be found on pages 38, 45, 59, 70, 83, 96 of the caffein report.)

The comparison of the various sets of results are interesting and show in a more concrete manner than is ordinarily possible in drug experiments the difference between various types of influence.

The alcohol effects maintain the same direction and increase in magnitude with the increase in dose. The alcohol effect on the pulse rate is considerably less than that produced by the dinner, and the control has no effect on pulse.

The greatest effect of the alcohol in terms of per cent of forenoon score is on Steadiness. All the agents, whether Control, Dinner, Alcohol, or Caffein, produce unsteadiness. The unsteadiness following three or four bottles of beer is quite like that following the dinner, and the effect of the larger and the special beer doses is less than that produced by the six grains of caffein, the amount of caffein contained in about two cups of coffee.

In the case of Coördination, Color Naming, and Opposites, the effects of Control, Dinner, Small and Large doses of Beer, and both amounts of Caffein are quite comparable in amount though often dissimilar in direction.

In Adding and Tapping the effects of the small and large beer doses are not unlike those of the dinner in amount, but opposite in direction. These effects are, however, both larger in amount and different in direction, as compared with the effects of the caffein doses.

The special beer doses may be considered separately because of the peculiar conditions characterizing them. These, it will be remembered, represent records taken under conditions of bodily distress and illness, consequent, in the main, upon the consumption of such large amounts of cold liquid in so short a time. The changes are in the same direction as those produced by smaller amounts of beer, and in Tapping and Adding there is no important difference in amount. The Pulse acceleration is still less than that produced by the dinner and the unsteadiness is less than that caused by the six grains of caffein. On all the remaining processes the effect is definitely greater either than that produced by the Control, the Dinner, the smaller amounts of Beer, or the Caffein doses.

X

THE MEMORY EXPERIMENT

On the last three days of the experiment one of the experimenters (Professor Woodworth) found time to prepare a further test which was used on June 17, 18, 19, these being, respectively, the three-bottle Beer day, the Special Alcohol day, and the Dinner day. This was a test for immediate memory by the method of "paired associates." At each trial a list of pairs of words was read to the subjects by the experimenter at a moderately slow rate, and with alternately short and long pauses to indicate the pairing of the words. The words themselves were chosen in a random way, so as not to utilize in any particular way established associative bonds between the members of the pairs.

After the list had been read the first word of each pair was spoken by the experimenter, and the subjects wrote on record slips the companion word for each stimulus word, as previously heard in the test list. On the first day there were twenty pairs of words in the lists used, and on the second and third days the lists contained fifteen word-pairs. Inasmuch as no records are available

for Blank days or for Control days, these records can be utilized only by comparing afternoon with forenoon performance, using the Dinner day as an approximate control for the two alcohol days.

No attempt was made to secure lists of equal difficulty at each trial. On June 17 and 19 four forenoon and four afternoon trials were recorded. On June 18 there were three forenoon and four afternoon trials. Although averaging four lists does not guarantee elimination of inequalities, it tends in that direction, and examination of the records shows that at least the various lists were of approximately equal difficulty. The scores of one individual on various lists do not vary more than do the scores of different individuals for the same list.

Comparison is also made insecure through the fact that the June 17 lists had twenty word-pairs, whereas the lists on the other two days had fifteen. The peculiar dependence of memory on amount of material does not entirely justify the expression of the results in the form of percentages, although this is perhaps the fairest comparison that can be made of the data here available. The data are presented in spite of these various qualifications, because the apparent results are so striking. They at least suggest the probable importance of this type of test in experiments with alcohol, and make it desirable that such tests be used more fully in later work. In order to avoid ambiguity the complete data for the three days are shown in the following table, and the conclusions may be checked up by reference to them, without reliance on averages and percentages of dubious reliability.

It is clear from inspection of these individual records that the afternoon records are inferior to forenoon records on all three days. The afternoon loss is least on the Dinner day, and it is greatest on the Special Alcohol day. If we express the afternoon performance in terms of per cent of forenoon performance and average the results for all six subjects, we may secure three figures which perhaps best summarize the data. On the afternoon of the Dinner day the loss is 9 per cent, on the three-beer day the loss is 22 per cent, and on the Special Alcohol day the loss is 60 per cent. The difference between the Dinner day and the three-beer day is not significant because of the roughness of our original data, but the very great difference between these two and the Special Alcohol day cannot be doubted. But since we have no records for Control nor for Blank days, it is impossible to say precisely how much of this effect is due to alcohol and how much to a natural afternoon decline.

TABLE 4

DATA FOR MEMORY FOR PAIRED ASSOCIATES.

Trial	1	2	3	4	5	6	Average	
				June 17, List of twenty pairs.				
1	3	9	2	7	2	8	5.2	
2	4	3	6.5	10.5	3	2	4.8	5.3
3	4	6	6	11	2.5	4	5.6	
4	5	9.5	6	6	2	5	5.6	
				Dose — three bottles beer.				
5	3	0.5	1	4	2	5	2.6	
6	5	5	3	5	0	8	4.3	4.1
7	3	4	4	8	1	2	3.7	
8	5	9.5	11	9.5	1	7	5.7	
				June 18, List of fifteen pairs.				
1	4	12.5	7	5	4.5	7	6.7	
2	4	12.5	5	8	1.5	7	6.3	6.2
3	..	..	..	..	..	..	..	
4	4	8	3.5	5.5	3	9	5.5	
				Dose—Special Alcohol Day.				
5	4	4	3.5	6	2.5	5	4.2	
6	3	1	3.5	2	0	0	1.6	2.5
7	4	Absent	1.5	3	1	2	2.3	
8	4	1	2	1	2	2	2.0	
				June 19, List of fifteen pairs.				
1	7	10	5	7	3.5	10	7.1	
2	5	7	9.5	10	9	12	8.7	8.2
3	6.5	10.5	5	7	9.5	10.5	8.2	
4	3	13	8	8.5	8.5	12	8.8	
				Dose — Dinner				
5	7	11	3	6.5	7	10.5	7.5	
6	5	13	8.5	5.5	Absent	Absent	8.0	7.5
7	5	11	6	6	9.5	13	8.4	
8	4	9	7	6	4.5	7	6.3	

XI

INDIVIDUAL DIFFERENCES

The conspicuous differences in the effects of alcohol on the six subjects may be exhibited in a fairly clear way by verbal summaries of their characteristic reactions.

Subject I is influenced by small doses in Steadiness and Substitution only. In Color Naming neither dose is effective, and in the other tests he reacts only to large doses.

Subject II reacts in all tests (with the possible exception of Opposites) to both small and large doses, and in some tests reacts also to Control doses.

Subject III reacts to both doses, except in Color Naming and Opposites, where neither is effective, and in Adding, where only the large doses have any effect at all.

Subject IV shows no clear effects from either dose in Opposites, Tapping, and Adding, reacts in Substitution only to large doses, and in the other tests reacts to both doses but with relatively slight effects.

Subject V shows conspicuous effects from both doses in all tests except in Adding, where only large doses are effective.

Subject VI gives no clear reaction to either dose in Steadiness, Colors, Adding. Large doses only produce changes in Substitution, Opposites, and Tapping. Coördination shows from both doses effects which are temporary with complete recovery.

On the basis of these descriptions, and without considering the quantitative effects in any precise way, it is easy to rank the subjects according to their susceptibility to these doses. Subjects II and V are clearly most easily affected. Subjects IV and VI are most immune or resistant. Subjects III and I constitute an intermediate pair, with "moderate" susceptibility. In the following tabulation they are arranged in order of decreasing susceptibility, and certain other items are given in each case.

TABLE 5

Subject	Age, Years and Months	Weight, Pounds	Height, Inches	Pulse, Initial	Exercise	Alcohol Habits
II	25–6	106	64.5	82	Inadequate	Occasional Moderate
V	22–7	147	72.5	73	Irregular	Regular Moderate
III	25–1	147	67.5	66	Vigorous	Abstainer
I	29–5	139	70.0	68	Moderate	Occasional
IV	22–11	182	74.5	92	Strenuous	Occasional Moderate
VI	21–5	149	69.5	74	Active	Regular Considerable

It is of course to be understood that no conclusive statements as to the basis of individual differences in general can be made on the basis of so few cases, nevertheless such suggestions as the table affords are not without interest.

If we compare the three pairs of subjects, no consistent difference in age is found paralleling the degree of susceptibility. Nor do previous alcoholic habits have any clear relation to degree of resistance. Subject VI, the most resistant, is, to be sure, also the most regular user, but he is also the youngest, one of the most active, and next to the heaviest. The median morning pulse at the first trial of each of the first nine days bears no relation to the degree of resistance.

But height, weight, and habits of physical exercise seem to bear

some relation to resistance. Thus the average weights of the three pairs, in order of increasing immunity, are 126, 143, and 165 pounds. The average heights are 68.5, 68.7, and 72.0 inches. The members of the first pair report themselves as taking no regular physical exercise, or very little. The members of the second pair exercise moderately or vigorously. Those of the third pair report strenuous and active outdoor exercise. That is, on the whole, degree of susceptibility seems to vary inversely with height, weight, and amount of exercise. The dependence of drug effects on body weight are of course well established. Perhaps that is after all the only real factor that is involved here.

More important than any of these factors, however, seems to be the individual's limit of capacity in the tests themselves, and his capacity for gain. In all the tests the individuals differ appreciably, both in the beginning and at the end of the investigation. Thus the following table gives, among other things, for each man in each test, the median score in the last two mornings on which six trials each were taken. The score is thus the median of twelve trials close to the very end of the experiment and represents very well the level of achievement finally reached by each man.

On the basis of these scores the men may be ranked on the basis of their relative final proficiency. In the table these ranks are given for the various subjects arranged in order according to their susceptibility to the effects of alcohol.

There is a very striking negative correlation between final proficiency and susceptibility to the effects of alcohol. The ranking for susceptibility is almost exactly the reverse of that for proficiency, the actual coefficient of correlation being by the method of rank differences, —.77.

The only tests that do not show this relation are Steadiness and Coördination, in which there is only a low correlation between final proficiency in the test and general susceptibility.

This relation is not due to a statistical fallacy, *e.g.*, to large differences in individual level with a consequently more obvious absolute change present in the case of standards which are themselves of great magnitude. All of these subjects have very closely approximated their physiological limits in each of the processes, and the absolute scores, though easily distinguishable, do not differ greatly in magnitude. In no test except Steadiness is the best score twice as good, numerically, as the poorest. The suggestion is very clear that individuals of relatively high competence are less liable to impairment in their proficiency through the influence of alcohol than are those whose constitutional capacity is

TABLE 6

INITIAL AND FINAL ABILITY; ABSOLUTE AND PER CENT GAINS

(+ means Gain; — means Loss in efficiency,)

Subject	Steadiness		Tapping		Coordination		Substitution		Colors		Opposites		Adding		Summary		Measure
	Score	Rank	Score	Rank	Score	Rank	Score	Rank	Score	Rank	Score	Rank	Score	Rank	Total Ranks	Final Rank	
II	16	3	444	5	117	6	61	6	59	6	73	6	121	6	38	6	Initial
	13	4	398	6	141	6	75	6	60	6	55	6	72	6	40	6	Final
	+3	4	—46	6	+24	4	+14	6	—1	6	+18	4	+49	3	33	6	Gain
	+19	5	—14	6	+20	4	+23	5	—2	6	+25	6	+40	5	37	6	Gain %
V	16	3	452	4	132	2	81	3	49	4	58	2	74	1	19	2	Initial
	12	3	498	4	161	2	100	4	50	4	37	3	60	4	24	4	Final
	+4	3	+46	3	+29	2	+19	4	—1	5	+21	5	+14	6	28	5	Gain
	+25	4	+12	3	+22	1	+23	4	—2	5	+36	3	+19	6	26	5	Gain %
III	16	3	468	2	138	1	76	4	48	3	62	3	111	4	20	3	Initial
	14	5	502	3	167	1	106	3	46	3	45	5	58	2	22	3	Final
	+2	6	+34	5	+29	1	+30	2	+2	4	+17	6	+53	1	25	4	Gain
	+13	6	+7	5	+21	3	+39	2	+4	4	+27	5	+48	1	26	4	Gain %
I	7	1	415	6	118	5	61	5	56	5	63	4	112	5	31	5	Initial
	4	1	485	5	142	5	80	5	52	5	39	4	61	5	30	5	Final
	+3	5	+70	2	+24	5	+19	5	+4	3	+24	2	+51	2	24	3	Gain
	+43	2	+17	2	+20	5	+31	3	+7	3	+38	2	+45	2	19	2	Gain %
IV	24	5	497	1	126	3	95	2	47	2	53	1	102	3	17	1	Initial
	5	2	598	1	154	3	136	1	42	2	35	2	60	3	14	1	Final
	+19	1	+101	1	+28	3	+41	1	+5	2	+18	3	+42	4	15	1	Gain
	+79	1	+21	1	+22	2	+42	1	+11	2	+33	4	+41	4	15	1	Gain %
VI	24	6	467	3	124	4	103	1	47	1	68	5	84	2	22	4	Initial
	14	6	507	2	146	4	123	2	39	1	32	1	46	1	17	2	Final
	+10	2	+40	4	+22	6	+20	3	+6	1	+36	1	+38	5	22	2	Gain
	+42	3	+9	4	+18	6	+20	6	+13	1	+53	1	+45	3	24	3	Gain %

inferior. The correlations, in the case of each of the tests, are shown in the following table, in which are also given correlations with ability to gain as the result of practice. Correlation coefficients based on so few cases have of course very low reliability, so far as their magnitude is concerned. They are nevertheless used here as a convenient rough indication of the direction of the relationships.

TABLE 7

CORRELATIONS OF SEPARATE TESTS

	Final Skill and Susceptibility to Alcohol in General	Absolute Gain and General Susceptibility to Alcohol
Steadiness.	—.09	—.49
Tapping.	—.77	—.55
Coördination. . . .	—.09	+.37
Substitution. . . .	—.77	—.60
Color Naming. . .	—.83	—1.00
Opposites.	—.83	—.71
Adding.	—.71	+.43
Med.	—.77	—.55

Table 6 gives for each man in each test his median record on the first forenoon of the regular series of days, and his median record during the last two full forenoons of work. Between these two records 84 trials intervened in each case, enough to show very clearly any differences in tendency to gain through practice. The gain in score is given in each case, in whatever absolute units of measurement were used in the test. Since the basic scores do not differ by large amounts, these absolute gains may be used as a measure of the individual capacity for improvement. To further control the results, however, the relative gain is also given, and the rank on this basis. By relative gain is meant the percentage which the absolute gain is of the initial record.

Final ranks are given in each case, based on the individual average ranks for all the tests. These final ranks in the last column of the table show that on the average there is but little change in rank as the result of practice. Subjects II, III, I, and IV occupy the same ranks at beginning and end of the series. Subjects V and VI change places. On the whole, then, those with high initial capacity tend to maintain their relative status through sufficient improvement.

The ranks for absolute gain also tend to show that those who have higher competence make larger absolute gains, in spite of the handicap of their original high scores. Percentage gains show the same tendency. Perhaps a better statement of improvability

is secured by noting the tendency of each man in each test to improve or lose in rank. The following table gives the change in rank for each man in each test, and the general tendency for each man as shown by the algebraic sum of his gains and losses in position in the seven different tests. The result is very striking.

TABLE 8
GAINS IN RANK

Subj.	Stead.	Tap.	Coord.	Subst.	Color	Opps.	Add.	Net Gains, Algebraic Average	Rank
II	−1	−1	0	0	0	0	0	−2	5
V	0	0	0	−1	0	−1	−1	−3	6
III	−2	−1	0	+1	0	−2	+2	−1	4
I	0	+1	0	0	0	0	0	+1	3
IV	+3	0	0	+1	0	−1	0	+3	2
VI	0	+1	0	−1	0	+4	+1	+5	1

The individuals who are least susceptible to the alcohol effects tend to gain in relative standing at the expense of those who are more susceptible. There is almost a one to one relation between position for susceptibility and position for gain or loss in relative standing through practice. The rank order correlation between susceptibility and gains in rank is —.94.

In table 7 are given the correlations between rank in absolute gain and rank in susceptibility. The correlations are negative except in Coördiation and Adding, where they are positive instead. Ability to gain in Color Naming gives a perfect negative correlation with susceptibility to alcohol effects in general.

The tendency is even more pronounced than these figures indicate, since the most competent individual, by increasing his score, is not able to gain in rank. Nor does the inferior individual with the lowest score lose in rank by making less gain than others. This "end error," which affects all expressions by the method of ranking, works in this case against the true tendency.

These two general results of the study of individual differences are extremely suggestive. Among this group of subjects, who, to be sure, are few in number, resistance to the effects of alcohol varies directly with competence, not only in the tests themselves but also in capacity for improvement through practice. Greater susceptibility to the effects of alcohol characterizes the individual with inferior competence and inferior ability to gain through repeated trials.

Although it is not possible to generalize these findings by extending them to cover all cases of idiosyncrasy in drug reactions, there is other evidence that points in the same direction. In another connection the writer has been able to show ("Psychology of Functional Neuroses," D. Appleton, New York, 1920) a very similar relation between general mental competence and liability to chronic psychoneurotic symptoms. Soldiers returned to this country during the recent war with obstinate psychoneurotic complaints are found to be more inferior in general intellectual competence than the average soldier. Furthermore, the more overt and physical the symptoms, the lower is the average mental level. Educational records show also that this level represents a pre-war status. There can be no doubt that relative mental inferiority predisposes one to the development or manifestation of psychoneurotic symptoms. General mental competence is negatively correlated with susceptibility to psychoneurotic symptoms.

XII

COMPETENCE AND SUSCEPTIBILITY, IN PREVIOUS STUDIES

As a matter of fact, such results as those we have just shown have already been hinted at in the literature of pharmaco-psychology, although the point has never, to my knowledge, been developed. In that classical summary of the results of his ten years of preoccupation with the study of drugs, Kraepelin observes that individuals differ considerably in their susceptibility to such drugs as chloroform, ether, and alcohol, and speculates concerning the constitutional basis of these differences. He remarks that in the case of three reaction time subjects the one with slowest normal reaction was the most susceptible to the "paralyzing effects" of the drug. In another connection he remarks: "If we arrange the subjects in their most probable order for susceptibility to alcohol (in the tests of learning, reading, and adding) from the results of both tables (20 and 30 gr. doses) . . . except for De these positions are the same as those for fatiguability. . . . In general it would appear that there is a relation between susceptibility to alcohol and individual fatiguability. The greater this latter the less marked are the stimulating effects of alcohol, the smaller the doses required to produce impairment, and the greater the range of this impairment." He suggests further that this relation is quite intelligible if we consider that in both cases we are dealing with the susceptibility of organs to the effects of poisons (fatigue

products and alcohol). As tentative as these suggestions are, they point clearly toward the possible importance of the general quality of the organism, along with other factors, in determining the individual reactions to drugs.

I have undertaken to analyze the individual data of several drug studies that have been recently reported, in the endeavor to determine to what degree susceptibility and incompetence go together in these cases. Limitations of space make it impossible to give the numerical results of these analyses here, but the general conclusions are extremely interesting. Unfortunately it is not usually possible to give individual records in full, so that there are not many investigations, on several subjects, by comparable methods, in which such analysis can be made from the literature.

A recent report by Schilling ("Effect of Caffeine and Acetanalid," Psychological Review, Jan., 1921), giving results on twenty subjects, affords an interesting opportunity to study individual differences. The only relevant point in this connection reported by this investigator relates to the absence of difference between smokers and non-smokers. But fortunately he gives the average records for each subject in each experiment, so that it is possible to work over the data from the point of view of our present interest. Both drugs retard reaction time, as compared with the control dose, on the whole, but the tendencies of the twenty subjects vary considerably. The subjects may readily be classified into groups of ten each or of five each, according to their normal speed of reaction. In the case of both drugs, and no matter whether absolute or relative effect be considered, the retardation shown by the quick group is small, but that shown by the slow group is considerable. It is the least competent whose work is most disastrously affected by the drugs.

Carver ("Psychological Effects of Tobacco Smoking," Journal of Compar. Psychol., Aug., 1922) has reported experiments on groups of subjects, from eight to twelve in number, in various performances. Three of the tests, Card Sorting, Adding, Multiplying, are reported for a sufficient number of subjects, and in adequate quantitative form for the study of individual differences. That such idiosyncrasies exist is emphasized by this investigator but no indication is given of the rules covering these idiosyncrasies. I have grouped the subjects in these tests into subgroups, on the basis of their efficiency in the tests under normal conditions, and then noted the tobacco effects on the various subgroups. The three tests are entirely consistent, and consistent also with our own findings. It is those subjects who excel in the

normal performance of the task who best resist the effects of the drug. Drug resistance is positively correlated with general competence.

From my previous caffein study I have taken the results from three tests in which a drug effect was shown and in which the number of subjects was as many as ten—Tapping, Color Naming, and Steadiness. The rule holds for Color Naming and Steadiness, but not for Tapping. Averaging the three tests, the rank orders for normal competence and degree of drug influence give a correlation that is negative (—.68).

Somewhat related to these general results are the findings of Allport in his study of the influence of a social group on the work of the individual. The group was found in general to favor more rapid work in such processes as free chain associations and argumentative thinking. Individuals differed in their susceptibility to the stimulating influence of the group, and Allport concludes that "there is a tendency for the slow individuals to be more favorably affected in speech by the group activity than the more rapid workers" (Allport, F. H., "Influence of the Group," etc., Jour. Exper. Psych., June, 1920, p. 180). This result is quite in accord with our present findings, viz., greater susceptibility to the agent characterizes the inferior individual.

Since most of the drugs considered in these surveys have been seen to result in impaired performance, we may in a sense look upon them as obstacles or distractions, or, in Woodrow's terminology, "detractors." I recall this investigator's terminology since I wish to refer to some of the results of his studies of the topic of attention. Woodrow* shows that not only do individuals differ in the prolongation of reaction time occasioned by the change from uniform to irregular "fore-periods" (intervals between "Ready" signal and stimulus to reaction) but that the individuals show characteristic degrees of detraction with different stimuli and with different forms of reaction task. Using the degree of change in reaction time as a measure of attention, he concludes that attention is a general trait in which individuals differ consistently. In the place of the word detraction we may use our own term, "susceptibility," the susceptibility being to the detracting influence.

Now an interesting fact about Woodrow's data is not mentioned by him, namely, not only is it true that individuals who are very susceptible in one situation are correspondingly so in others, but it is also true that the susceptible individuals are those whose

* The Faculty of Attention. Journ. Exper. Psychol., Aug. 1916.

normal reactions are slow—that is, the susceptible are the incompetent. Dividing his group of subjects into Quick, Fair, and Slow subjects (normal reaction time), with four in each sub-group, gives the following average results:

	TOUCH		SOUND		LIGHT	
	Normal	% Distract.	Normal	% Distract.	Normal	% Distract.
Quick....	125 s	39%	142 s	42%	175 s	36%
Fair.....	153	46	165	43	209	39
Slow.....	183	62	199	66	235	61

Or we may correlate the normal reaction times with either absolute or with relative (per cent of normal) detraction, and the following coefficients result, by the method of rank differences:

	Correlations of Normal Reaction Time, with Degree of Detraction, to Different Stimuli		
	TOUCH	SOUND	LIGHT
Absolute Detraction.	.89	.87	.95
Per Cent Detraction.	.63	.70	.85

It is very clear that it is the poor reactors who are the most susceptible to the damaging effects of the detracting influence. And this is in spite of the fact that we might expect just the opposite result. We might, that is, suppose that poor performance was in part the result of perfunctory work and that such reactors might maintain their original level of excellence without difficulty even under the detractor. Instead the results are precisely such as we have found in the case of susceptibility to the influence of drugs.

XIII

COMPENSATORY FACTORS

Although there have been many comments on "idiosyncrasy" and "individual differences" in the reaction to drugs, little light has ever been thrown on these differences. It would be an interesting result if it should ultimately be shown that "general intelligence" is one of the factors that underlie such idiosyncrasies. If it be true that relative mental incompetence not only predisposes the individual to psychoneurotic complaints but also renders him relatively more susceptible to the effects of foreign toxic substances taken into the system, Kraepelin's early faith in the drug experiment may yet be justified.

Even if relative mental incompetence is not the basic fact, but only a third trait, along with psychoneurotic predisposition and susceptibility to the effects of drugs, the evidence of positive correlation between these traits is important. However the relations

be interpreted, it would have such a profound bearing on our general notions of character and personality that the question merits the more thorough investigation to which, in another connection, we are submitting it. Even if, as may be suggested, it should be true simply that the more competent individual is therefore more able, through autogenic reinforcement, to overcome the maladaptation occasioned by shock, strain, or toxic agent, the demonstration of the fact is none the less valuable. Thus Dodge and Benedict remark (p. 246): "In spite of sleepiness, pain, or sensory distraction, and even narcosis, one can reëstablish the normal controls on occasion, and make a fair showing, especially when the results would be serious if one let oneself go. . . . Reinforcement in these cases seems to consist chiefly of an arousal to more or less normal performance."

There is in fact some evidence of compensatory factors such as this in the case of these six alcohol subjects. This evidence is to be found in the effect of the alcohol on pulse rate. In the following table are given the median pulse records of all subjects under various conditions. In the first column are given the medians of the medians of the first five pulse records on each of nine forenoons, this being then the normal median forenoon pulse. In the next four double columns are the median afternoon pulse rates for blank days, control days, small alcohol, and large alcohol days. These rates are also expressed as percentages of the forenoon medians.

Subtracting the control percentage from that of the alcohol percentage gives the true alcohol effect. This is shown in later columns for small and large doses separately. In the final two columns are given the ranks for pulse change and for susceptibility to alcohol in the tests.

There is a clear negative correlation between pulse change and change in the tests. Subjects IV and VI, who are least susceptible according to the tests, are most susceptible according to pulse rate. Subjects II and V, who show greatest changes in test records, show least changes in pulse rate. The intermediate Subjects I and III occupy the same intermediate ranks for pulse change. The correlation by the method of rank differences is —.83.

If there is any reason for believing that pulse rate reflects the degree of effort, the suggestion is clear that Subjects IV and VI resisted the disastrous effects of alcohol on the test performances by autogenic reinforcement, that is, by making special effort. Subject V, on the other hand, shows practically no alcohol effect on pulse, and II shows it only after large doses. Does this mean

TABLE 9

Subject	Forenoon Median	Afternoon Blank Days Median		Afternoon Control Days Median		Afternoon 3–4 Beer Median		Afternoon 5–6 Beer Median		True Alcohol Effect Alcohol–Control Per Cent		Rank for Pulse Change	Rank for Susceptibility
	Beats	Beats	%	Beats	%	Beats	%	Beats	%	3–4 Beer	5–6 Beer		
1	60.4	56.0	93	58.0	96	61.2	101	64.2	106	+5	+10	4	4
2	71.2	64.1	90	67.4	95	68.0	96	76.1	107	+1	+12	5	1
3	58.6	54.2	92	53.4	91	60.5	103	59.4	101	+12	+10	3	3
4	80.0	69.2	87	64.5	81	78.0	97	75.0	94	+16	+13	1	5
5	67.0	64.2	96	64.9	97	65.8	98	66.0	98	+1	+1	6	2
6	68.4	65.1	95	68.4	100	74.5	109	80.9	118	+9	+18	2	6

that they made the usual effort only, and that hence their work suffered?

On the other hand, an entirely different explanation may be suggested. The increased pulse may represent the primary effect, which had as its correlate improved work in the tests, thus neutralizing the primary alcohol effect on performance. In the absence of such compensation in the case of II and V, the tests suffered. So far as the present investigation is concerned there seems to be no way of determining which of these alternatives is correct, whether, that is, reinforcement is present with the subjects least affected in the tests, and, if it be, whether it is autogenic and primary or reflex and secondary.

It is to be borne in mind throughout this discussion that the subjects were under oath to make their best effort at every trial. Every incentive was afforded that would further this maximal effort, and there is no reason to doubt that such maximal effort was really made by every subject at every trial.

Whatever explanation of the negative correlation be accepted, it is clear that it adds a further detail to the question of idiosyncrasy. Idiosyncrasy cannot be inferred from the work product alone, nor from such neuromuscular changes as underlie pulse rate alone. The ranks of our six subjects in these two respects are quite reversed.

If now we attempt to summarize this examination of idiosyncrasy for alcohol, it would be in some such way as the following: Among this group of adults those individuals whose work is most susceptible to the effect of alcohol are shorter in stature, lighter in body weight, less given to active exercise, less competent in the work itself, less able to gain in competence through practice, and their pulse rate is less changed under the influence of alcohol. Individuals who are taller and heavier and more disposed to vigorous exercise show less alcohol effect in their work. Individuals who are relatively more competent in their work, and who also show greater capacity for improvement through practice, are less influenced by alcohol in their work, but manifest a greater change in pulse rate. That other factors are involved in that vague characteristic traditionally known as idiosyncrasy is not denied. But that the factors here analyzed out of the complex are important elements in determining individual differences in susceptibility to alcohol at least seems undeniable. Moreover, a reëxamination of several other sets of data, accumulated in the study of other drugs, suggests that the factors are similar in those instances as well. Drug resistance is positively correlated with general competence.

THE EFFECT OF AN AUDIENCE UPON PERFORMANC

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INTROSPECTIVE reports from both trained and untrain observers indicate that the presence or absence of an audie frequently has an effect upon performance. Some individu describe an exhilaration which they feel when talking or worki in the presence of others; many persons confess to vari difficulties in muscular coördination, in habitual speech reactio or in the higher mental processes; finding, perhaps, that th knees shake, their hands tremble, they hesitate, forget words, are unable to solve promptly the simple problems which ar Observation of students working in a laboratory seems to indic that the experiment often suffers when the instructor or oth are looking on. It is frequently recommended that, in order t the results of oral pyschological tests be accurate, they be gi with no observers except the questioner present.

This study reports an attempt to measure quantitatively assumed effect of an audience. The field of investigation limited definitely to (1) the effect of spectators on a certain gr of subjects, in this case, college women; (2) to the influence particular kind of audience—fellow students and an instru of the subject; (3) to the reaction caused by the mere pres of the observers not of anything they might do in the wa friendly or unfriendly behavior; and (4) to the effect of particular audience on these particular subjects in a few si motor or associative processes.

Three groups of subjects were used, a control group of 25 took all the tests in the presence of the experimenter on "small audience" group of 26 who took the first half of the with the experimenter alone present and the second half o tests before from 4 to 6 observers, and a "large audience" g of eleven who performed the first half of the experiment u conditions identical with those of the other groups, and the s half of the tests in the presence of an audience of from 27 spectators.

All three groups were given individually the following in order: the Coördination (Three Hole) Test, (the record being the number of contacts made in one minute); the W worth-Wells Color-Naming Test; one form of the Woodw Wells Analogies Test; a vocabulary test, the record of whic

imply the number of nouns named in one minute. Then, after a hort pause, during which (in the case of the two audience groups) he audience was introduced into the situation, a second trial was iven of the Coördination, Color-Naming, and Analogies Tests, sing a different form of the last, and a request to name as many djectives as possible in one minute.

In the case of those individuals belonging to the "small audi-nce" group, the experimenter said, after the first four tests had een completed in her presence alone, "I want you to show these eople how you do these tests," and opened the door admitting he audience. After taking their seats in a semi-circle close to the ubject, the spectators did not speak or smile but kept their eyes ixed on the performer's face in an attitude of courteous atten-ion. They nodded politely in response to any attempt of the ubject to engage them in conversation and to the experimenter's uestion "Can you see the subject all right?" which occurred at he completion of the first test and usually produced some evidence f embarrassment, a smile or a blush, on the part of the performer.

The "large audience" group were also given individually the irst four tests, and then were brought before one of the classes n psychology, and asked to repeat the tests, seated on the plat-orm facing the audience. In order to keep the attention of the pectators upon the performers, the former had been instructed o write down, at the end of each individual's tests, all the expres-ions of emotional disturbance which the observers had been ble to detect. The subject who looked up from her task always found the eyes of the class fastened upon her.

In Table I the results of these observations of behavior in the ase of three subjects tested before a class in Elementary Psy-chology are given. The records are of value only in showing he great variability of report obtained when such instructions are given. The reader has great difficulty in determining just how the subjects did behave. Did Subject I blink, finger her collar, or her hair, laugh, sigh, stutter, wet her lips, did her hand shake or was her voice low, or did all of these things occur? In each case only one observer reports the reaction as significant. There is in fact, only one case (an observation of "hesitation") where more than one-fourth of the individuals interpret the same bit of behavior as a sign of emotional disturbance. Even when a more general comment on behavior is included, judgments vary greatly. For example, two people say that Subject II shows a total disregard of the audience, one says that she is very nervous; five people say that Subject III is very conscious of the

audience, one says that she is more at ease than any of the others; one person notes no sign of nervousness in Subject I in whom 57 signs of emotion have been observed by others. Plainly, neither these reports of specific behavior nor the more general interpretations given by one or perhaps by a number of observers could be taken as a measure of the effect of this situation upon performance. Lack of a standard of comparison for interpretation—since many of the reactions noted may be merely habits occurring

TABLE I—SIGNS OF EMOTION NOTED FOR THREE SUBJECTS BY THIRTY-ONE OBSERVERS

Subject I		Subject II		Subject III	
Emotional Expressions	No. of Times Observed	Emotional Expressions	No. of Times Observed	Emotional Expressions	No. of Times Observed
bit lips	6	bit lips	5	bit lips	2
blinked	1	blushed	1	bit nails	5
fingered collar	1	breathless	1	caught breath	2
fingered ring	6	clenched hands	1	cleared throat	1
fixed hair	1	fixed glasses	4	face flushed	2
hand shook	1	fumbled	1	faster breathing	3
hands clenched	3	giggled	1	fidgety	1
hesitation	4	hands shook	5	frowned	2
laughed	1	hesitation	13	fussed with hair	2
looked away from audience	3	nervous smile	7	gasped	1
low voice	1	repetition of words	3	giggled	2
nervous smile	4	shook head	1	hand shook	5
played with finger	1	set mouth	1	hesitated	6
sighed	1	sighed	1	laughed	3
stuttered	7	stuttered	3	made faces	1
swallowed	3	swallowed	3	moistened lips	2
tense position	3	tense	1	nervous breathing	1
wet lips	7	slightly conscious of audience	1	nervous smile	3
no sign of nervousness	1	total disregard of audience	2	pressed lips together	1
		very nervous	1	tense	2
				voice strained	3
				watched audience	5
				winked	2
				winked eyebrows	1
				more at ease than any of others	1
				very conscious of audience	5

whether the individual is disturbed or not—is one difficulty; another seems to be the subtle character of some of these external signs combined with the varying distances and points of view of the observers.

In the case of the psychological tests it is possible to compare the three groups in initial ability when all conditions were identical and to contrast this with their performance when subjected to varying circumstances. Though certain differences which may be of significance occur, the groups are approximately

TABLE II—AVERAGE PERFORMANCE OF THE THREE GROUPS IN INITIAL TEST.

Group	Coördination		Color-Naming		Analogies		Naming Words	
	Average no. of Taps in 1 minute	S. D. of the Distribution	Average time in seconds	S. D. of the Distribution	Average time in seconds	S. D. of the Distribution	Average no. of nouns named in 1 minute	S. D. of the Distribution
Control	88.8	11.2	54.0	7.9	57.2	13.9	48.0	6.4
Small Audience	88.1	11.2	57.4	7.3	64.0	12.5	44.6	7.8
Large Audience	91.0	20.3	49.8	6.0	61.6	11.2	42.8	3.8

equal in the initial tests as is shown by Table II. The control group is always slightly better than the "small audience" group, the "large audience" group being the best of all in two tests, the worst in one, and occupying a middle position in the other.

Table III shows the amount of improvement made by these groups, similar in initial ability, and subjected to varying stimuli in the second tests. With but one exception all groups gain. The loss is slight and occurs in the case of naming words, where it was necessary to adopt a different procedure for measuring improvement, as the second test was more difficult than the first. In order to equate the scores of the two, the average loss for the control group was computed and subtracted from each of the individual measures. The control group, then, is considered to

TABLE III—AVERAGE IMPROVEMENT OF THE THREE GROUPS.

Group	Coördination		Color-Naming		Analogies		Naming Words	
	Average no. of Taps in 1 min.	S. D. of the Distribution	Average time in seconds	S. D. of the Distribution	Average time in seconds	S. D. of the Distribution	Average no. of nouns named in 1 min.	S. D. of the Distribution
Control	5.7	7.3	3.9	3.3	7.7	14.6	0	4.3
Small Audience	6.2	8.5	2.9	2.6	3.9	9.4	—.9	8.7
Large Audience	4.4	11.0	2.4	4.6	6.5	9.5	3.3	8.5

make an equal score in naming nouns and naming adjectives, and the records for the other groups are given in terms of words named in excess of or less than this zero point.

The significance of the difference in improvement made by the three groups is shown in Table IV. The average difference in gain made by any of the groups is less than the S.D. of the difference in eight of the twelve cases, the two are approximately equal in the other four. In the simplest test (coördination) the equality is most obvious. In the most complex test (naming

TABLE IV — THE DIFFERENCE BETWEEN THE GAINS MADE BY THE THREE GROUPS.

	Coördination		Color-Naming		Analogies		Naming Words	
	Difference	S. D. of the Difference	Difference	S. D. of the Difference	Difference	S. D of the Difference	Difference	S. D. of the Difference
Between the Control and the Small Audience Groups	—.5	2.3	1.0	.9	3.8	3.4	.9	1.9
Between the Control and the Large Audience Groups	1.3	3.6	1.5	1.6	1.2	4.1	—3.3	2.8
Between the Large and Small Audience Groups	1.8	3.7	.5	1.5	—2.6	3.4	—4.2	3.1

words) the only difference which might be of significance is a slight indication of stimulation on the part of the "large audience" group.

There is a possibility that, though the differences themselves are too slight to be statistically reliable, some significance may be attached to the uniformity of their direction. The control group makes a larger improvement than the "small audience" group in three of the four cases (in the exceptional case the differences are very small), it makes a greater gain than the "large audience" group in all but the word-naming test. The "small audience" group is better than the "large audience" group in just half the

ases. Since the control group was slightly better at the begin-ing than the "small audience" group we might expect on other vidence, that if all conditions were identical, it would improve ess. The direction of the difference might give some indication, hen, of an effect of the audience too slight to be reliably esti-

TABLE V — PERCENTAGE OF SUBJECTS WHO IMPROVED, FELL OFF, OR REMAINED THE SAME IN THE THREE GROUPS.

	Improved	Fell Off	Remained the Same
Coördination			
ontrol	.84	.08	.08
mall Audience	.77	.04	.19
arge Audience	.73	.00	.27
Color-Naming			
ontrol	.96	.00	.04
mall Audience	.77	.08	.15
arge Audience	.73	.00	.27
Analogies			
ontrol	.76	.04	.20
mall Audience	.65	.04	.31
arge Audience	.73	.00	.27
Naming Words			
ontrol	.48	.44	.08
mall Audience	.42	.58	.00
arge Audience	.82	.09	.09

nated by our measures of difference. The control group, in the majority of the cases gains slightly more than the other two, though no difference is found between the group tested before from 4 to 6 and the group tested before from 27 to 36 observers.

This is borne out by Tables V and VI which show, respectively, the percentage of subjects which improved in each test in each group, and the percentage of subjects which improved in four, three, two, one, or none of the tests. In all cases, but one (the

TABLE VI — THE RELATION BETWEEN IMPROVEMENT IN ONE FUNCTION AND IMPROVEMENT IN THE OTHERS.

	Control	Small Audience	Large Audience
Percentage of group which improved in 4 tests only	32	08	46
Percentage of group which improved in 3 tests only	48	54	18
Percentage of group which improved in 2 tests only	12	30	27
Percentage of group which improved in 1 test only	08	08	09
Percentage of group which did not improve in any test	00	00	00

TABLE VII — THE EFFECT OF THE AUDIENCE UPON GROUPS DIFFERING IN INITIAL ABILITY

	Average Improvement in Taps	Percentage of Subjects Who Improved
Coördination		
Control		
Highest 8 Subjects	7.3	88
Middle 9 Subjects	4.4	78
Lowest 8 Subjects	5.6	88
Audience		
Highest 8 Subjects	1.5	63
Middle 10 Subjects	10.4	90
Lowest 8 Subjects	5.5	88
	Average Improvement in Seconds	**Percentage of Subjects Who Improved**
Color-Naming		
Control		
Highest 8 Subjects	3.6	100
Middle 9 Subjects	3.3	88
Lowest 8 Subjects	4.9	100
Audience		
Highest 8 Subjects	1.9	75
Middle 10 Subjects	3.6	90
Lowest 8 Subjects	2.9	88
	Average Improvement in Seconds	**Percentage of Subjects who Improved**
Analogies		
Control		
Highest 8 Subjects	2.3	63
Middle 9 Subjects	4.6	66
Lowest 8 Subjects	16.6	100
Audience		
Highest 8 Subjects	—5.4	25
Middle 10 Subjects	3.6	70
Lowest 8 Subjects	12.5	100
	Average Improvement in Seconds	**Percentage of Subjects who Improved**
Naming Words		
Control		
Highest 8 Subjects	—2.5	50
Middle 9 Subjects	—1.3	33
Lowest 8 Subjects	5.3	63
Audience		
Highest 8 Subjects	—2.6	37
Middle 10 Subjects	—2.1	30
Lowest 8 Subjects	1.4	63

word-naming), in Table V the largest number showed a gain in the control group. But, again, these differences are slight. In each case the change of from one to three individuals from gain to loss or *vice versa* would reverse the direction of the difference, The same tendency is shown again slightly in Table VI where the evidence in favor of the control group is found by averaging the first two figures and the third and fourth figures. A larger percentage of the control group improve in all or three of these tests, a smaller percentage gain in half or less. In no group is there an individual who fails to improve in any test.

Table VII is an attempt to discover whether the audience has any differential effect upon groups varying in initial ability. If there is some result of the presence of spectators which we can discover, is this shown most clearly by those who were originally poor or those who are originally good? In Table VII the control and "small audience" groups only are divided according to initial ability and the effect of the audience in each division noted.

In the case of the middle group, the results are ambiguous; in the lowest group, the differences are slightly in favor of the control group. When we examine the best records, however, we find a decided tendency for the audience group to improve less than the control group, in all tests but the word-naming. Perhaps it is the initially better individuals who tend to be affected unfavorably by the presence of spectators.

The data of this study show, then, a great variability of report when observers are asked to note expressions of emotional disturbance in others. The results suggest that the mere presence or absence of spectators has a very slight effect, if any, on these particular subjects in these particular functions. No reliable differences are obtained between groups tested in the presence of but one observer and groups tested before an audience of from 4 to 6 or from 30 to 40 spectators, except in the case of the most difficult test, that of naming adjectives where there is a slight possibility of the existence of a stimulating effect due to a large audience. If it is possible to use as evidence the direction of slight differences, we might argue from their consistency, which is evident when we consider either the amount of improvement, or the percentage of subjects improving in all or any one test (but the word-building) that the performance of the groups which were subjected to the stimulus of the audience was in general made less efficient by this condition. There is a suggestion that the better individuals in three tests are disturbed more than are the average and poor subjects by the presence of spectators.

The results found are similar to those obtained in an investigation of the effect of encouraging or discouraging remarks upon performance [1] where, again, very slight differences, which in this case tended to be in favor of the encouraged group were found. The data may be considered to be consistent with those of Laird [2] who found a decrease in motor control when an audience was permitted to "razz" subjects, inasmuch as he added a stimulus provocative of disturbance to the one investigated here. The experimenter plans to repeat the observations using more difficult tests (such, for example, as the Peterson Rational Learning Test) where greater concentration of attention is needed; since the simpler processes already tested seem to be affected so slightly by the mere presence of observers.

EDITORIAL COMMENT UPON THE EFFECT OF AN AUDIENCE

There are a few points of interest in a comparison of the results of the foregoing study with other investigations. With the kind permission of Dr. Gates these suggestions are added herewith.

It has been pointed out above that the originally superior subjects are on the whole those most unfavorably affected by the audience. If we compare the results of the highest and lowest subjects in the "audience" group, not with equivalent classes in the control group, but with each other, the differences dependent upon original ability stand out vividly. The following table presents this comparison from the data given.

INFLUENCE OF THE AUDIENCE WITH REFERENCE TO INDIVIDUAL DIFFERENCE OF ABILITY

Test	Average Improvement in Terms of Test Scoring Unit		Per cent of Subjects Who Improved	
	Highest 8 subjects	Lowest 8 subjects	Highest 8 subjects	Lowest 8 subjects
Coördination	1.5	5.5	63	88
Color-Naming	1.9	2.9	75	88
Analogies	—5.4	12.5	25	100
Word-Naming	—2.6	1.4	37	63

[1] Gates and Rissland. The Effect of Encouragement and of Discouragement upon Performance. *Journal of Educational Psychology*, Vol. XIV, No. 1.

[2] Laird, Donald A. Changes in Motor Control and Individual Variations under the Influence of Razzing. *Journal Experimental Psychology*, Vol. VI, No. 3.

It will be seen that in every case, both in average improvement and per cent who improved, the lowest eight subjects gained more than the highest through the influence of the audience. The difference is striking in the per cent of subjects showing improvement in the tests of analogies and word-naming. In two cases the average of the highest group was actually lowered in the period under observation by the audience. It is true that the lowest subjects of the *control* group tend to show the same superiority in improvement after the *rest* period. This tendency, however, is not nearly so marked as in the other case. In "average improvement" it occurs in only three of the four tests, while in "per cent who improved" it occurs in only two tests. No doubt these results in the control group may be explained by the fact that the poorer subjects were those who were at first unfavorably affected by working in a group; but who after a period of habituation were spurred to greater effort by the sights and sounds of those working about them.

But notwithstanding this situation in the control group, the quantitative difference between the improvement of the lowest as compared with the highest subjects in the control group and the corresponding improvement in the audience group seems to indicate that in the latter the audience itself affects the inferior individuals more favorably than it does the superior.

Other investigators, such as Mayer, Moede, and the present writer have found a frequent inverse correlation between original ability and tendency to improve under the influence, not of an audience, but of the "co-working" group itself. That is, working *together* tends to benefit the work score of the slower reactors more than it does that of the quicker reactors, as measured in comparison with work done *alone*. This is particularly true when the setting involves competition, a condition no doubt true to some extent of all co-active performance. I have elsewhere interpreted this phenomenon as due largely to differences of rapidity of stimulations from the work of the quicker and slower workers respectively, combined with the rivalry incentive. We have from Dr. Gates' study the interesting suggestion that the presence of *inactive onlookers* also may spur the slowest workers, while it has little influence (and sometimes a deleterious one) upon the speed of the more rapid. It may be that the audience renders the slower workers more conscious of their slowness, and increases their incentive to rival, and perhaps excel, their fellow workers.

It should be borne in mind that working in a group has been repeatedly found to stimulate an increase in quantity of indi-

vidual output. No doubt such co-working in the fairly large groups of Dr. Gates' experiment had already raised the level of effort to a point much nearer the physiological maximum than would have been the case if the individuals had been tested alone. Under such circumstances the audience may have had the effect in some cases of over-stimulating and impairing muscular coördination and mental work (cf. Triplett's findings upon "over-rivalry" in children). In other words we have here the resultant of two social influences, fellow-workers and an observing audience, rather than the isolated effect of the latter condition. It might be worth while to repeat Dr. Gates' method using solitary individuals, in one set of trials working wholly unobserved, and in another set working before an audience.

For references bearing upon the above suggestions consult the following:

Mayer, A., "Ueber Einzel und Gesamtleistung des Schulkindes," *Archiv für die Gesamte Psychologie,* 1903, I, 276-416.
Moede, W., "Der Wetteifer, Seine Struktur und sein Ausmass," *Zeitschrift für Pädagogische Psychologie,* 1914, XV, 353-68.
Triplett, N., "The Dynamogenic Factors in Pace-making and Competition," *American Journal of Psychology,* 1897, IX, 507-32.
Allport, F. H., "The Influence of the Group upon Association and Thought," *Journal of Experimental Psychology,* 1920, III, 159-182; also, *Social Psychology,* ch. 11.

F. H. A.

JUNG'S "PSYCHOLOGICAL TYPES" AND WILL-TEMPERAMENT PATTERNS

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IN studying certain patterns of profile obtained from will-temperament testing[1] I have frequently been impressed with the possibility of describing the subjects giving them as introverted or extraverted. Other investigators have remarked the same possibility. It seems, therefore, worth while reviewing certain passages in Jung's "Psychological Types" in order to determine the extent to which such a parallelism holds.[2] The approach in Jung is theoretical; that in will-temperament testing experimental; a comparison of conclusions would, therefore, be particularly instructive.

Extraversion, Jung defines "as an outward-turning of the libido. With this concept I denote a manifest relatedness of subject to object in the sense of a positive movement of subjective interest toward the object. Everyone in the state of extraversion thinks, feels, and acts in relation to the object, and moreover in a direct and clearly observable fashion, so that no doubt can exist about his positive dependence upon the object."[3]

"Introversion means a turning inwards of the libido, whereby a negative relation of subject to object is expressed. Interest does not move toward the object, but recedes toward the subject. Everyone whose attitude is introverted thinks, feels, and acts in a way that clearly demonstrates that the subject is the chief factor of motivation while the object at most receives only a secondary value."[4]

In other words in extraversion the movement of attention is toward the environment, social or physical; in introversion away from it.[5]

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[1] "Will-Temperament and its Testing," World Book Co., 1923.
[2] "Psychological Types," Harcourt, Brace & Co., 1923.
[3] Loc. cit., p. 542.
[4] Loc. cit., p. 567.
[5] I do not agree wholly with Conklin's interesting formulation of the difference between introverted and extraverted attitude (this JOURNAL, Jan.-Mar., 1923) in terms of the relative domination of attention by subjective or objective conditions. The attention of the extravert may be very largely determined by social meanings, social values, social principles, as an outcome of tradition, education, social pressure, all of which are subjective conditions of attention. The contrast, as I see it, is rather, a matter of orientation toward the world of ideas and of things leading in the case of the extravert to relatively immediate EXPRESSION and in the case of the introvert to MENTAL (IMAGINAL?) ELABORATION. Jung's types remind me of the older distinction between motor and sensory types.

Jung, it should be observed, makes much less of the existenc of so-called types than one would think from reading his critics. He writes: "Introversion and extraversion are not *character* at all, but *mechanisms,* which can, as it were, be inserted or dis connected at will. Only from their habitual predominance do th corresponding characters develop. There is an undoubted predi lection depending upon a certain inborn disposition, which how ever, is not always absolutely decisive for one or other mecha nism."[6]

Again, "Every human being possesses both mechanisms as a expression of his natural life-rhythm. . . . A rhythmical alterna tion of both forms of psychic activity may correspond with th normal course of life. . . . Outer circumstances and inner dis position frequently favor the one mechanism, and restrict o hinder the other; whereby a predominance of one mechanis naturally arises. If this disposition becomes in any way chroni a *type* is produced, namely an habitual attitude, in which the on mechanism permanently dominates; not, of course, that the othe can ever be completely suppressed, inasmuch as it also is a integral factor in psychic activity."[7]

The closest point of contact between Jung's Types and wi temperament patterns is to be found in Jung's discussion of th type-problem in psychiatry[8] where he devotes himself largely an analysis of the types exploited by Otto Gross in his study the secondary cerebral function.

Without going into the question of the value of the hypothes of a secondary cerebral function it is at least evident that the emerges from Gross' discussion a clear-cut picture of contrasti behavior types as follows:

(1) Where there is rapid recovery of the primary functi there is high "réagibilité," rapid adaptations, and changes attitude. The psychological picture shows "a constant and rapid renewed readiness for action and reaction, hence a kind *capacity for deviation,* a tendency to superficiality of associati connections,"[9] etc. Quick inflammability and rapidly fadi enthusiasms accompany rapid reactions. With a prompt grasp the immediate present there occurs compliancy, a facile glidi over meanings, the appearance of presence of mind and audaci Thinking tends "more towards a quick presentation and orde

[6] Loc. cit., p. 354.
[7] Loc. cit., p. 13.
[8] Loc. cit., chap. VI.
[9] Loc. cit., p. 339f.

arrangement of contents than towards abstraction and synthesis." In extreme cases we get "inferiority with shallow consciousness." The normal—and less extreme—individual is the typical extravert.

(2) When the secondary function is particularly intensive and prolonged, "The idea has an enduring effect; the impression goes deep. One disadvantage of this is a certain limitation within a narrow range, whereby thinking suffers both in variety and abundance." [10] But "restriction to one thema undoubtedly effects an enrichment of the relevant associations and a firm cohesion and integration of the complex"—a type which Gross calls the sejunctive individual results from such an organization. There is inner seclusiveness. "An accumulation of stimuli becomes unbearable whence violent defense reactions appear." A chronic defense attitude may result. The individual belonging to this type is at a loss when presence of mind or promptness of reaction is required; there are faulty adaptations in social situations. In extreme cases we get "inferiority with contracted consciousness." The normal—and less extreme—individual is the typical introvert.

To repeat: the first type shows a shallow extensive consciousness; the second type, a narrow intensive consciousness. The first type is a practical one because of quick adaptation to the environment and the wholesale taking over of ready-made ideals and ideas; the second is at a disadvantage in a novel situation, but reaches a more original and profound solution of problem-situations.

Gross' first type of individual corresponds very nicely to a characteristic reaction on the will-temperament test described as a rapid-fire or fluidic reaction-pattern. This reaction-pattern exhibits quick reactibility, little load, and much flexibility. Frequently, compliance is shown by a mild reaction to contradiction and considerable capacity for enthusiasm is suggested by the results from a motor impulsion test. There is little interest in detail and not much perseveration.

Gross' second type suggests a slow, deliberate reaction-pattern obtained in the will-temperament test. There is slow reactibility, little flexibility, little compliance (strong reaction to contradiction) and persistent reaction to opposition. This type is unable to adjust quickly to a somewhat complex situation (coördination of impulses) but shows great interest in detail and long perseveration.

[10] Loc. cit., p. 341f.

To be sure, clear-cut patterns of reaction on the will-temperament test are relatively infrequent. And, to repeat, pure extraverts and introverts are only limiting conceptions. "I have no desire," says Jung, "to give my readers the impression that such pure types exist at all frequently in actual practice. They are, as it were, only Galtonesque family-portraits, which sum up in a cumulative image the common and therefore typical characters, stressing these disproportionately, while the individual features are just as disproportionately effaced."[11]

Let us next look a little more closely at certain traits of the will-profile and examine them with reference to the concepts of extraversion-introversion. Speed in movement and speed in decision appear to be symptoms of extraversion; slowness of movement and in decision symptoms of introversion. But slowness in decision is more characteristically an introvert trait than slow movement since decision is more relevatory of the subject than is movement, at least habitual movement; and caution, according to Jung, is the real reason for the introvert's apparent slowness rather than actual lack of speed.[12]

Much inertia (or as Gross calls it, tension) characterizes the introvert and for the same reason as slowness of reaction. The extravert shows little load or tension. Flexibility is an extravert trait, lack of it an introvert one.

Speed of movement and decision, freedom from load, and flexibility constitute together a group of four will-temperament tests which when scored high suggest the typical extravert, just as failure to make average scores on them suggests the typical introvert. The four traits just listed make up the first group of tests of the will-profile. It is profitable to note in this connection that patients suffering from dementia precox—a typically introvert disorder—run excessively low on this group of traits. Frequently it is impossible to score them at all. My records for about twenty patients of this type suggest only one exception to this statement, —the patient was of the paranoid variety.

For another group of will-temperament tests (tests of motor inhibition, interest in detail, coördination of impulses, and volitional perseveration) there is less pronounced parallelism with Jung's types. Interest in detail and volitional perseveration appear to be characteristic traits of the introvert and lack of these traits characteristic of the extravert. Both extravert and intro-

[11] Loc. cit., p. 513.
[12] Cf. Chap. IX.

vert may, however, fail in the so-called "coördination of impulses" test of the will-temperament series. The first is inclined to sacrifice accuracy to speed; the second, speed to accuracy. Dementia precox patients show a complete incapacity to handle the "coördination of impulses" test and no interest in detail; but they give high scores on the perseveration test.

Reactions to the so-called tests for aggressiveness in the will-temperament series cannot be used to parallel introverted and extraverted traits in any clear-cut fashion. The four traits grouped together under this general rubric are motor impulsion, reaction to contradiction, resistance to opposition, and finality of judgment. The rapid-fire individual (extravert) under distraction yields to automatism and produces a large free handwriting; the inhibited individual (introvert) produces a small cramped handwriting. The first reacts tactfully to contradiction and the second cautiously or brusquely. The rapid-fire individual resists opposition quickly and more or less forcefully; the inhibited individual blocks at first, but as soon as he has sized up the experimental situation resists opposition strongly. The rapid-fire person may have little power of motor retardation so far as deliberate control is concerned but he may be able to slow down his movements by adopting a somewhat automatic mental set; the introvert may or may not possess power of motor retardation. So far as dementia precox patients are concerned they do not give on the aggressive traits a typical deliberate reaction. They react stubbornly to contradiction, with frequently an initial flare-up, but they do not resist opposition with any energy and they show no power of motor retardation.

Types are, as Jung insists, infrequent occurrences. I should prefer to think of them as descriptive tools, classificatory in function. They serve a purpose in helping us to think a world together and in pointing out fields for profitable exploration of causes. They need not block our appreciation of the infinite variety of individuals.

SUGGESTIBILITY AND NEGATIVISM AS MEASURED BY AUDITORY THRESHOLD DURING REVERIE[1]

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INTRODUCTION

THE purpose of this study is to determine experimentally a means whereby suggestibility and its antithesis, negativism, can be discovered and measured.

It is highly important to discover psychotic trends in normal individuals before the advent of an actual break, so that the environmental situations can be modified, possible precipitating factors removed, and the individual saved from an actual psychosis. A test which could indicate such trends could be applied in elementary schools to determine what pupils should be given special treatment and to indicate the actual nature of that treatment. In the histories of many cases of psychoses, it is found that certain peculiarities developed early in the life of the patient, but that they were not taken into account in the patient's education.

The primary object of this experiment is to discover a ready means to classify children in two important directions and to furnish a prophylactic guide.

The data have been gathered primarily from the abnormal field, for there the psychoses are already determined, and the reactions are indicative of certain types of abnormal disorders. Now if it is found that the psychoneurotics respond one way to a definite situation and the dementia precox patients another way, and that certain normal individuals respond like the psychoneurotics while certain others react like the dementia precox patients, it does not seem illogical to infer that something in common has been indicated and with proper methods can be measured. It is a well-established fact, as will be brought out in the historical survey, that the psychoneurotics and especially hystericals are suggestible and that the schizophrenics are negativistic. Certain criteria can be mentioned by which to judge whether a person is suggestible or negativistic, but to our knowledge no objective measurements are

[1] This paper reports an experimental study carried on in the laboratory of the Psychological Clinic at the State University of Iowa under the direction of Professor John J. B. Morgan, Director of the Clinic. The author wishes to acknowledge his gratitude to Dr. Samuel T. Orton, Director of the Psychopathic Hospital, for the privilege of working with patients and using the case histories.

in existence. If a test is devised to obtain the reaction of an hysteric, who we know is suggestible, and the response of a dementia precox patient, who we know is negativistic, and these responses are found to be diametrically opposed, it seems safe to conclude that a measurement of the predominant characteristic of each has been made. Further, if some normal people give a response to a situation like the hysterics give, and other normal individuals give a response like that of the dementia precox patients, then it appears that a likely statement of the chief characteristic of each group of normal individuals could be stated objectively.

What is lacking is an objective, numerical indication of the two traits under discussion. We know to what the traits, if possessed to an extreme degree, may lead, and for this reason it is important if not imperative for school teachers to be able to measure them in normal individuals.

At present there is no absolute proof that a normal individual who responds to a situation like a schizophrenic does will, in the event that a psychosis is developed, come within the schizophrenic group, or that a normal person who reacts to a situation like the hysteric does will, in the case that a psychosis is developed, fall within the class of hysterical. But when the responses of the normal and the abnormal are alike, and these responses are made to a situation which touches a vital element of personality, such as introversion and extroversion, one is led to believe that the possible trend can be predicted. Psychopathologists know from the pathological viewpoint, which is gained from a study of the abnormal field where the introvertive and the extrovertive are already determined, that certain environmental circumstances enhance introversion, and that others augment extroversion. If in the normal the introvertive or negativistic and the extrovertive or suggestible could be determined, those factors which tend unduly to increase either, as are known in the abnormal field, could be withdrawn or withheld.

Thus the problem is to determine the suggestible or the negativistic child and thwart the impending abnormal development, and prevent a probable psychosis. It is comparatively easy in the abnormal field to determine the negativistic and the suggestible after the crystallizing factors of the environment and the traumatic episodes of an active life have had their effects. But before the individual shows marked traits of either of these characteristics it is important to know which he possesses in order that prophylactic measures can be adopted.

HISTORICAL SURVEY

The aim of this survey is to show that the two opposed traits, suggestibility and negativism, are recognized as present in an extreme degree in the abnormal field, and that they indicate certain contrary psychoses, represented on the one hand by schizophrenia and on the other by hysteria; and also to point out that the importance of these two traits has not been appreciated in the normal field, neither as to their present nor potential significance.

The works reported are those of authors who are experts in the field. More could have been drawn upon, but neither space nor interest would have warranted such a collection. The opinions of Ferenczi, Bleuler, Abraham, Kraepelin, Jastrow, Freud, and Janet shall furnish the material for this particular phase of this research.

In several of the discussions it must be borne in mind that paranoia and dementia precox are treated as characterized by the same mechanisms, and both fall under the larger group, paraphrenia. Ferenczi makes the following statements:

"The dement completely detaches his interest from the outer world and becomes auto-erotic. The paranoiac, as Freud has pointed out, would like to do the same, but cannot, and so projects onto the outer world the interest that has become a burden to him. The neurosis stands in this respect in a diametrical contrast to the paranoia, whereas the paranoiac expels from his ego the impulses that have become unpleasant, the neurotic helps himself by taking into the ego as large as possible a part of the outer world, making it the object of unconscious phantasies."[2]

In another passage the two are clearly characterized:

"So finally there appear the opposite characters of the large-hearted, impressionable, excitable neurotic, easily flaming up with love of all the world or provoked to hate of all the world, and that of the narrow-souled, suspicious paranoiac, who thinks he is being observed, persecuted, or loved by the whole world. The psychoneurotic suffers from a widening, the paranoiac from a shrinking of his ego."[3]

The next quotation is extremely vital for the purpose of this study as it is suggestive of one of the main contentions set forth in the data and held in the conclusions. It follows:

"When we review the ontogenesis of the ego-consciousness on the basis of the new knowledge, we come to the conclusion that the

[2] Ferenczi, S., "Sex in Psychoanalysis." Authorized translation by Ernest Jones. Richard G. Badger, Boston, 1916. pp.47.

[3] *Ibid.*, p. 50.

paranoiac projection and the neurotic introjection are merely extreme cases of psychical processes, the primary forms of which are to be demonstrated in every normal being."[4]

There is just one fault to find with the above statement, and that is, the demonstration cannot refer evidently to any objective, numerical validation of the principle set forth. The conclusions to be drawn from Ferenczi's remarks, that the mechanisms of the neurotic and of the paraphrenic are diametrically opposed, need objective verification and, that they are present in primary forms in every normal individual, demands experimental proof. To make such verification and proof is the aim of this research.

According to Bleuler, one of the causes of external negativism of the dementia precox patient is "the autistic withdrawing of the patient into his fantasies, which makes every influence acting from without comparatively an intolerable interruption."[5]

Abraham expresses the same observation in somewhat different terminology:

"It (libido) is turned back upon the ego and this reflected turning back is the source of the megalomania in dementia praecox."[6]

The conditions which are observed by Kraepelin in the domain of dementia precox are so complex and the general behavior of the psychic activities so varied that only those statements which bear directly on our problem will be used. In the two following quotations the outstanding characteristic of one of the groups under discussion is clearly stated and should be authoritative coming from Kraepelin, the outstanding student of dementia precox:

"Negativistic obstruction of volition plays therefore an extraordinarily large rôle in the clinic picture of dementia praecox. To begin with it leads to the intrinsic suppression of all reaction to external influences, further to stubborn opposition to interference of all sorts, and in the end to the performance of actions which are suggested by the circumstances or required by the environment."[7]

"It is common experience that the patients with dementia

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[4] *Ibid.*, p. 55.

[5] Bleuler, E., "The Theory of Schizophrenic Negativism." The Journal of Nervous and Mental Diseases Publishing Company, New York, 1912.

[6] Abraham, K., "The Psycho-sexual Difference between Hysteria and Dementia Praecox," 1908.

[7] Kraepelin, Emil, "Dementia Praecox and Paraphrenia." E. & S. Livingston, 17 Teviot Place, Edinburgh, 1919. pp. 47.

praecox are more or less inaccessible, that they shut themselves off from the outer world."[8]

The more one comes in contact with cases of schizophrenia and hysteria, the more one is convinced that they embrace two diametrically opposed sets of characteristics. The one abnormality is characterized by suggestibility, splitting of consciousness, and dissociation. The other is introversion, shattering of the personality and negativism. Concerning the features of the hysteric, Freud remarks:

"The more we occupied ourselves with these phenomena the more certain became our convictions that the splitting of consciousness, so striking in the familiar classical cases of double-consciousness, exists rudimentarily in every hysteria, and that the tendency to this dissociation, and with the tendency towards the appearance of abnormal states of consciousness which we comprehend as 'hypnoid states' is the chief phenomenon of this neurosis."[9]

Of all that Janet has said about hysteria only one of his statements will be selected, as it is not within the scope of this study to deal with somnambulism, dissociation, hypnosis, and allied topics, but rather to adhere strictly to the existence or absence of suggestibility in certain cases of mental disorders:

"In a word, my opinion on this point has become more and more definite. Suggestion is a precise and relatively rare phenomenon; it presents itself experimentally or accidentally only in hystericals, and, inversely all hystericals, when we study them from this standpoint, present this same phenomenon in a higher or lower degree. If we add that, as we shall see later, this psychological fact plays a great rôle in the formation of their disease, we may say that the most important mental stigma of hysteria is suggestibility."[10]

Other writers beside those who are especially interested in the subject of mental diseases have recognized the outstanding features of hysteria. Jastrow, for example, in writing concerning the subconscious, says that hysteria is characterized by "exaggerated impressionability" and "a passionate indulgence in extravagant daydreams."[11]

To what does this historical survey lead? It shows that some mental disorders are characterized by negativism, while others

[8] *Ibid.*, p. 49.

[9] Freud, Sigmund, "Selected Papers on Hysteria and Other Psychoneuroses." The Journal of Nervous and Mental Disease Publishing Company, 1919. pp. 8.

[10] Janet, Pierre, "The Major Symptoms of Hysteria." The Macmillan Company, New York, 1907. pp. 292.

[11] Jastrow, Joseph, "The Subconscious." Houghton Mifflin Company, New York, 1906. pp. 302.

are featured by suggestibility. The terms "negative" and "suggestible" are qualitative and general. What is demanded of modern psychology is specificity and objectiveness in describing human traits and behavior.

This study purports to determine a definite bit of behavior, characteristic of the hysterical type of individual on the one hand and of schizophrenic on the other, and in terms of the behavior thus discovered to classify normal individuals according to their modes of behavior as leaning toward the hysterical or the schizophrenic, as the case might be. It is undoubtedly true that the so-called normal individual falls somewhere between these two extremes. This experiment presents a definite situation which calls forth a definite response. The type of response varies according to the kind of organism making that response, and to determine the kind of individual exposed to the definite stimulation of this experiment the manner of response is taken as the criterion. If it is found that the hysterical responds to a certain situation with a lowered threshold, and a particular normal individual does likewise, then it seems safe to say that the normal individual is inclined to be suggestible. If it is also determined that the dementia precox patient reacts to the same controlled situation with a raised threshold, and a particular normal person responds likewise, it seems safe again to say that the latter is inclined to be negativistic.

It is hoped that these last statements will be amply proven as the thesis progresses.

APPARATUS AND METHOD

The apparatus used in this experiment is so simple and so easily set up that it should be serviceable for high schools and small colleges. For this reason the applicability of the whole experiment is greatly increased.

It consists of a tone generator, resistances to control the intensity of the tone, a receiver, by means of which the tone is heard by the subject, a key and buzzer circuit whereby the subject can respond when the tone is heard, a crystal to serve as an aid to the imagination of the subject, and a make-key for the experimenter.

The tone generator is of the direct current motor type driven by a three-celled Willard storage battery. The speed of the generator, and hence the pitch of the tone, can be varied by means of a rheostat, which is connected in series with the motor. However, a constant pitch of about 700 v. d. was used in this experiment. An electrically driven tuning fork could have been used with equal effectiveness and ease.

The intensity of the tone is controlled by a resistance box, which consists of three series of non-inductive resistances. One of the series ranges from 0 to 100 ohms resistance, another from 0 to 10, while the third ranges from 0 to .9. To make the intensity of the tone slight enough, in order to have a good range of subliminal tones, two resistance coils, one of 3040 and the other of 280 ohms, were placed in series with the receiver. The loudness of the tone can be varied and measured in terms of the resistances of the resistance box. The observer hears the tones through a watchcase receiver fastened to a head clamp.

The response circuit consists of three dry cells, a silent make-key, and a small buzzer.

The remainder of the apparatus comprises a clear crystal about

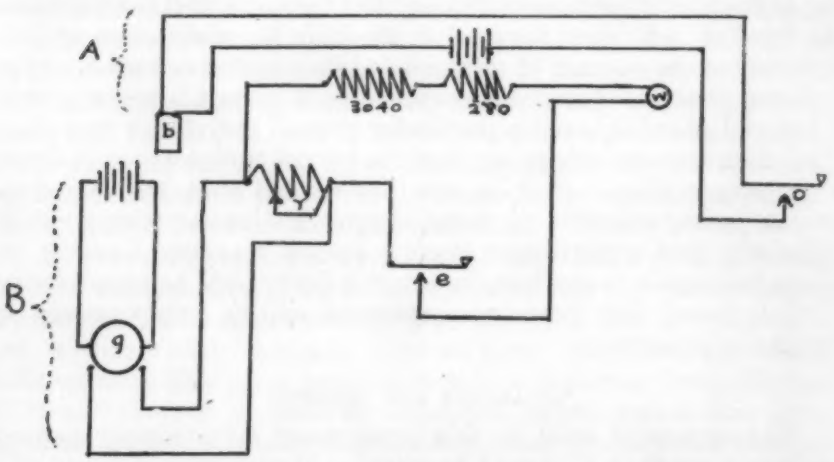

FIG. 1.—Drawing of the wiring used in this experiment. *A* is the response circuit; *B* is the tone circuit; *g* is the tone generator; *r* is the resistance box; *e* is the experimenter's key; *w* is the watchcase receiver; *o* is the observer's key; and *b* is the buzzer.

10 cm. in diameter mounted over a 40-watt blue mazda light bulb and a make-key for the experimenter. The silent key of the response circuit, the crystal, and the watchcase receiver are in the dark room, which is considerably removed from the laboratory, while the remainder of the apparatus is in the laboratory.

In order to make the above description of the apparatus more comprehensible, reference should be made to Figure 1. *G* indicates the tone generator, *e* the experimenter's key, *r* the resistance box, *b* the buzzer, *w* the watchcase receiver, and *o* the observer's key. The numbers 3040 and 280 represent the resistances in series with the receiver. When the generating motor *g* is running so

that a tone is being generated, the experimenter presses his make-key *e*, thus closing the circuit and causing a tone to be transmitted to the watchcase receiver *w* against the observer's ear. If the observer hears the tone, the intensity of which is regulated by shifting the switches over the terminals of the resistance box *r*, he presses his make-key *o*, thus closing the response circuit and causing the buzzer *b* to sound at the experimenter's elbow. When the buzzer sounds just after a tone has been given the observer is credited with hearing that tone. If the buzzer sounds when no tones have been given they are considered false responses. It is seen that there are two separate circuits, which will be termed the response circuit *A* and the tone circuit *B*. The pressing of the experimenter's key closes the tone circuit and produces a tone in the observer's receiver. The pressing of the observer's key closes the response circuit and causes the buzzer to sound.

The observer is seated comfortably in a rocking chair with the elbows resting on the arms. When the watchcase receiver has been firmly adjusted, the individual is placed snugly against a table of average writing height, on which is the crystal and the silent signal key. The mercury key is quite sensitive, but still some effort is necessary to make contact. Both the key and the crystal can be shifted so as to be most convenient, and the individual is always asked to make everything as agreeable as possible in order that distractions and annoyances might not interrupt the day-dreaming.

The threshold before the crystal-gazing is obtained with the lights on and the crystal removed from sight. The observer is told to attend strictly to the tones, taking special pains not to let the mind wander, and respond every time a tone is heard. He is told that the tones will become fainter and fainter, and is advised to concentrate fully on the task at hand. When this threshold is ascertained, the lights are turned off except the blue one under the crystal, the crystal is replaced, and the following instructions given. With some subjects, repetitions of some parts are necessary, and examples and illustrations of the meaning of certain terms are essential:

"Relax completely and forget all present surroundings, such as the receiver against your ear, the signal key beneath your finger, the chair in which you are sitting, and the table on which your arm is resting. Just day-dream and build air-castles, letting your mind wander wherever it will. Do not particularly listen for the tones which you have been hearing. Do not, however, resist them. Think that if they come your finger will press the key. Gaze stead-

fastly into the crystal, which will aid you to abstract. Do not permit your eyes to wander, but keep them fixed on the crystal. Suggest to yourself that you are drifting and floating away into a restful and relaxed condition of existence. Do not be afraid to lose yourself in reverie as you will not be disturbed for quite a long time."[12]

It is difficult adequately to describe the appearance of the crystal illuminated by the blue light beneath. Briefly it gives the appearance of the heavens on a clear, star-lit night. The whole setting of course is suggestive, and has such an effect upon the hysterical type of individual. The data indicate the influence of such an environment upon the other type of personality.

After the observer is left in the dark room for five or ten minutes subliminal tones are given. If responses are elicited the intensity of the tones is decreased until no responses are forthcoming. On the other hand, if the subliminal tones are not responded to, the intensity is increased until the individual does respond. In the case of the observer whose threshold lowers during crystal gazing, the faintest tone to which he will respond is taken as his threshold during that period. In the instance of the individual whose threshold rises during crystal gazing, the tone needed to elicit a response is taken as the threshold for the period. Let us illustrate this by two of the cases. The threshold of Case C before crystal gazing was found to be 7. During crystal gazing he was given tones of intensity 2 and 3 without any response being made. He responded, however, to three tones of intensity 4. Thus 4 is taken as the crystal gazing threshold of Case C. The threshold of Case K before crystal gazing was 25. During crystal gazing tones of from 10 to 35 intensity inclusive were given without bringing a response. To a tone of 36 intensity he responded,

[12] The above instructions are not entirely new, but were understood in bygone days. A classical example of this psychological method to induce dissociation will be found in a passage from the Gita in which Krishna gives directions to his worshipers. It reads as follows:

"A devotee should constantly devote himself to abstraction, remaining in a secret place, alone, with his mind and self restrained, without expectations and without longings. Fixing his seat firmly in a clean place, not too high nor too low (in order, evidently, that the mind should not be distracted by anything in the bodily attitude) and covered with a sheet of cloth, a deerskin and kussa grass, and there seated on that seat, fixing his mind exclusively on one point, with devotion for purity of self. Holding his body, head, and neck even and unmoved, remaining steady, looking at the tip of his own nose, and not looking about in various directions, restrain his mind and concentrate it on Me and sit down engaged in devotion, regarding Me as his final goal. Thus constantly devoting himself to abstraction, a devotee whose mind is restrained attains that tranquility, which culminates in final emancipation and assimilation with Me." (Translation by Telang, in S. B. E. American Edition, Vol. VIII, Part 2, pp. 68, 69.)

but then refused to respond to a tone of 35. Therefore 36 was taken as the crystal gazing threshold of Case K. After the crystal gazing period responses were made to all tones of intensity above 11. Therefore 12 was taken as the threshold after crystal gazing.

All tones are given in such a manner as to avoid any possible chance of the operation of a summation effect.[13]

After the crystal gazing threshold is verified by being ascertained two or three times during the crystal gazing period, which generally lasts from thirty to sixty minutes, the observer is interrupted, the lights are turned on and the crystal removed. The instructions given and the method used for obtaining the threshold after the crystal gazing period are the same as those for obtaining the threshold before crystal gazing. Care should be taken to make sure that the observer, especially one of the hysterical type, is thoroughly aroused, or the lowered threshold may continue to function in the following period. Day-dreaming is so pleasurable to some individuals that when it is once begun under such favorable circumstances as the crystal gazing situation affords, it may continue to operate in the post-crystal gazing period.

EXPERIMENTAL DATA

Table I shows the thresholds before, during, and after crystal gazing of fifteen abnormal cases represented by capital letters ranging from A to O inclusive. The diagnoses are those actually given or indicated by the psychiatrists of the hospital.

Table II gives the thresholds before, during, and after crystal gazing of ten normal individuals represented by capital letters ranging from P to Y inclusive. The chief characteristic of each personality is shown on the basis of a lowered or raised threshold during crystal gazing. P, Q, and R are doctors in the psychopathic hospital while the remainder of the normal observers are either juniors, seniors, or graduate students of the University.

Table III shows the direction of the change of the thresholds during crystal gazing and the number of judges indicating sug-

[13] It is evident that the summation effect did not enter in. A great number of those individuals, both normal and abnormal, whose thresholds lowered during crystal gazing, responded to the first subliminal tone given, and in no case was there more than six such tones made before considerable time was allowed to elapse. If the summation effect did operate its influence should have been apparent in the instance of those individuals whose thresholds rose, for this latter group always had the benefit of a great many more stimuli, varying from slight to greater intensity. When it is considered that the negativistic individuals demanded a tone far above the normal threshold to elicit a response during crystal gazing after they had been given a considerable number of tones varying from subliminal intensity to the required one, how can the summation effect still be maintained to operate?

gestibility, accessibility, and sociability or negativism, inaccessibility, and seclusiveness in the case of the ten normal individuals.

The thresholds of the first eight observers in Table I lowered during crystal gazing and these individuals, with the exception of G, who was left undiagnosed, were diagnosed as belonging to the psychoneurotic group, with five of them determined hysterics. The thresholds of the remaining seven rose during crystal gazing and all of them were diagnosed as dementia precox patients. In the case of those abnormal observers whose thresholds lowered

TABLE I

ABNORMAL SUBJECTS.

Patient	Thresholds[1]			Diagnosis
	Before	During	After	
A	30	15	20	Conversion hysteria
B	20	15	25	Quiescent congenital syphilis with juvenile anxiety hysteria
C	7	4	10	Mild involutional depression
D	6	1	7	Mild psychoneurosis
E	12	6	12	Conversion hysteria
F	8	3	8	Psychopathic personality with recent hysterical episode
G	20	11	35	Undiagnosed
H	33	23	25	Hysteria
I	6	60	8	Hebephrenic dementia precox
J	7	30	20	Paranoid dementia precox
K	25	36	12	Hebephrenic dementia precox
L	10	20	10	Paranoid dementia precox
M	20	60	13	Manic-depressive with symptoms indicating dementia precox
N	35	60	40	Dementia precox
O	1	3	1	Paranoid dementia precox

[1] These thresholds are in terms of ohms. The resistance was in parallel thus causing a larger number to indicate a louder tone.

during the crystal gazing period, responding did not cause their crystal gazing thresholds to come back to the ones determined before, as they continued to respond to subliminal stimulation during the crystal gazing period even after they had once responded, showing the set or attitude to be quite permanent and rigidly maintained for a considerable time. This fact can be made clearer by an actual case. The threshold of Case E before crystal gazing was 12. During crystal gazing he was given a tone of intensity 10 to which he responded. Now one might think that this responding

would arouse the observer and cause his threshold to return to 12. However, such is not the case. He now responded to 10, 8, and 6, but refused to respond to 4. Thus it is seen that responding does not effect the condition of the lowered threshold. The same fact holds true of the individual who experiences a rise in the threshold during crystal gazing. Responding does not change his set and cause the threshold to lower to the one determined before the crystal gazing period. The threshold of Case M before crystal gazing was 20. During crystal gazing 60 was necessary to elicit a response. Then 55 was given three times without being responded to. When 60 was given again a response was made.

TABLE II

NORMAL SUBJECTS

Observer	Thresholds			Chief Characteristics
	Before	During	After	
P	15	10	15	Suggestible
Q	6	2	15	Suggestible
R	12	10	12	Suggestible
S	13	11	13	Suggestible
T	11	5	10	Suggestible
U	14	8	14	Suggestible
V	12	15	12	Negativistic
W	13	15	13	Negativistic
X	12	16	12	Negativistic
Y	13	16	13	Negativistic

NOTE: P and Q were measured by means of a different rheostat than that used for the remainder of the normal observers, hence their thresholds are not at all comparable with the others. The rheostat used in the case of P and Q was also used with the abnormal individuals so that the thresholds of the former and the latter are comparable. It is not vital that the thresholds before, during, and after crystal gazing of one individual should be comparable with the thresholds before, during, and after of another individual. What is essential is that the threshold of one period can be compared to the threshold of another period in the case of the same individual.

The thresholds of the first six individuals in Table II lowered during crystal gazing, while the thresholds of the last four rose during the same period. The normal individuals indicated by capital letters from R to Y inclusive are taken from a study published by the author in 1922.[14]

[14] Travis, Lee E., "Studies in Dissociation." Journal of Experimental Psychology, Vol. V, No. 5, Oct. 1922. p. 338.

The thresholds during crystal gazing of some of the normal observers were affected by responding. However, even here it was rare that responding caused the crystal gazing threshold to return to the predetermined one. Only in the case of two normal observers whose thresholds raised during crystal gazing, did the first response or two place them in a normal state so that they would react to tones on the predetermined threshold. After the lapse of from five to ten minutes, however, their thresholds had gone back up again and a tone far above the pre-crystal gazing threshold was necessary to cause them to respond.

TABLE III

NORMAL SUBJECTS

Observer	Direction of Change in Threshold During Crystal Gazing	Number of Judges Indicating Suggestibility, Accessibility and Sociability	Number of Judges Indicating Negativism, Inaccessibility and Seclusiveness
P	Down	1	3
Q	Down	3	2
R	Down	5	0
S	Down	4	0
T	Down	5	0
U	Down	5	0
V	Up	0	0
W	Up	0	3
X	Up	0	4
Y	Up	0	5

The facts presented in Table III were obtained by having judges determine whether an observer whom they knew more or less intimately, was suggestible, accessible, and sociable or negativistic, inaccessible, and seclusive. Three of the judges were doctors in the hospital, two were graduate students, and one was a senior. The highest number of judgments for any one observer is five, as all of the judges except two had acted as observers and hence were disqualified to judge themselves.

The subject V who was not judged left school shortly after the experiment so that no opportunity was had to make a characterization in the light of intimate acquaintanceship. In these judgments the broader term suggestibility can be used to include accessibility, pronounced sociability, and extroversion. Likewise the term negativism can be used to include seclusiveness, inaccessibility, and introversion.

INTERPRETATION OF DATA

It was shown in the experimental data, Table I, that some abnormal individuals experience a lowering of the threshold during crystal gazing while others experience a rise; and that those observers who experienced the lowering of the threshold were of the psychoneurotic group, the majority being hystericals, while those abnormals whose thresholds rose were dementia precox patients.

In the historical survey, decisions from authors who are authorities in their respective fields were given to prove that psychoneurotics and especially the hystericals are more or less suggestible, and that the dementia precox patients are negativistic. The characterization of abnormal subjects adds weight to the conclusions of the historical survey by indicating the chief characteristics of the two types of abnormals. The dementia precox patient was found to be inaccessible, seclusive, disinterested, emotionally apathetic, and negativistic. The psychoneurotic was seen to be accessible, sociable, interested, emotionally excitable, and suggestible.

It was further shown in the experimental data, Table II, that some normal individuals have a lowered threshold during crystal gazing while others have a raised threshold, and in Table III, that those observers who had the lowered threshold were judged as possessing in varying degrees the characteristics of the psychoneurotic, and those who had a raised threshold were judged as possessing in varying degrees the characteristics of the dementia precox type.

In every case, whether the subject was normal or abnormal, individuals who showed psychoneurotic traits experienced a lowering of the threshold in this experiment, while all those, whether normal or abnormal, who had schizophrenic traits, experienced a rise of the threshold. This close relation between the schizophrenic and the psychoneurotic characteristics on the one hand and raised and lowered thresholds on the other, makes it safe to infer that if an unknown individual is given this test, a raised or lowered threshold will indicate that he is of the schizophrenic or psychoneurotic type respectively.

Now if some normal individuals lean toward the psychoneurotic group, and hence are more or less suggestible while others lean toward the schizophrenic group, and consequently are more or less negativistic, and further if these traits can be experimentally

discovered and measured, what can be said as to their real meaning?

They are undoubtedly learned just as any condition-response is learned. When a mother says "don't", and the child does the thing nevertheless, being rewarded by the act itself or by the indulgence on the part of the parent, the pleasure the child receives is interpreted as a direct result of the disobedience. In this manner a child can be taught that reward follows disobedience and the way to receive pleasure is to disobey. Furthermore if one parent in correcting a child is not supported by the other parent, the child is accordingly taught to disobey the former in order to receive the reward of the latter.

Another way that negativism can be developed is by teasing and nagging. It is a well known fact that dogs and ponies can be taught to be resistive, spiteful, contrary, stubborn, balky, and mean by this method. People familiar with horses immediately refer the cause of balkiness to bad training.

Just as a quiet and docile animal can be turned into a disturbing and vicious one by teasing and maltreatment, so also a gentle and good-natured child can be transformed into a stubborn and negativistic one by torment and nagging.

In these ways the characteristic of stubborness or negativism is begun and as time goes on, is strengthened. Whether or not a child possesses by original nature the trait of negativism, is not determined; but that this trait, howsoever it may arise, can be modified by environmental influences is demonstrable.

To what negativism may lead if possessed to an extreme degree is well brought out in abnormal cases. The dementia precox patient has developed the responses of his life so far that any stimulation from outside environment is an unbearable interruption. In order more efficiently to resist the influences from without, he entirely withdraws all interests from his surroundings and becomes the recluse *par excellence,* becoming dead so far as the things of this world are concerned. Even satisfying the most demanding and necessary physiological needs becomes an intolerable burden.

If the characteristics of this type of abnormality are learned or even exaggerated through training, then their presence in a normal individual means the potentiality for developing into such a mental disorder under undue stress. It is imperative that the trait be determined and measured early so that the proper training can be given which will modify the personality and avoid a possible psychosis.

It also can be shown that suggestibility is learned just as negativism is learned. It is taught by rewards being given only when the child accepts suggestions from the environment or by punishments being dealt only when the child fails to receive suggestions from his surroundings. Suggestibility really means dependence, as is illustrated by the individual who always relies upon another person for his decisions, modes of behavior or stimulation to endeavor.

Often in the case of twins, or of two children about the same age, one comes to depend upon the other and fails to develop a will of his own, being amenable eventually not only to the suggestions from his brother or sister, but from anyone.

A child begins by being permitted to lean too heavily on the mother or father, resting his future on their wisdom. The responses of incredulity, scepticism, and want of faith are not learned or exercised so that the individual comes to be susceptible to every environmental factor, good or bad, adequate or inadequate.

Suggestibility by no means indicates a lack of intelligence. It is not indicative of either the absence or presence of intelligence, as is amply proven by the intelligence quotients of highly suggestible individuals.

To what suggestibility, if possessed in an extreme degree, may lead, has been pointed out in various places in this study. The mental disorder characterized chiefly by suggestibility is hysteria. The hysteric has learned to take suggestions so unreservedly and whole-heartedly that he mirrors every slight play of environmental forces. He can be made to forget part of his life, and the use of part of his body by mere suggestion. He is ill or well, happy or sad, loving or hating, just according to the stimulation of external circumstances. Thus amnesias, paralyses, hallucinations, fantasies, and dream states are produced or removed by word of command.

In order to avoid the overdevelopment of a trait to the extent that it may lead the individual to make all sorts of maladjustments to the realities of life, it must be detected early in its formation and development so that it can be modified and guided.

It has been shown that the presence of these traits can be detected by raised and lowered thresholds. The question immediately arises as to why this is so.

The psychoneurotic and the suggestible type of normal individual indulge freely in day-dreaming which is the mildest form of dissociation and involves unrestrained association. In this state they become lost in revery, all restraint is disbanded and self-

control is lost sight of. Every attentive process and critical judgment is in abeyance so that automatic responses easily operate.

These facts are made clearer by recalling the method of experimentation. The subject's threshold before crystal gazing is obtained with the instructions to attend strictly and faithfully to the tones. As they become fainter he is told to apply greater effort in an attempt to hear them. Now the threshold during crystal gazing is obtained with the instructions to be neutral to the tone, to lose the self in reverie and to let the mind wander wherever it will. The suggestible person accepts these instructions fully so that the contrast in the mental processes of the two periods, before and during crystal gazing, is very marked.

The direction of attention before crystal gazing is on the stimulus, while during crystal gazing it is on the dream or phantasy of the reverie, making the responses during the latter period quite automatic. As is well shown in the literature on this subject, mild dissociation and hypnosis are accompanied by an increase in sensory acuity and speed in motor response, and it is also pointed out that if any responses are made during states of dissociation, they are to a greater or less exetent automatic. Thus the suggestible person while crystal gazing, if he responds at all, will respond more or less automatically; and as automaticity implies greater readiness to respond to stimuli than is ordinarily found in attentive conscious states, a lowered threshold is bound to occur.

Now why does the threshold of the dementia precox patient or the threshold of the negativistic normal individual rise during crystal gazing? Again let us recall the method of experimentation.

Before crystal gazing, the observer of the negativistic type has as low a threshold as the hysteric because he attends in about the same manner to the tone as does the suggestible person. But during crystal gazing, when the negativistic observer is told to day-dream, his attention becomes directed against any such procedure and he remains controlled by attentive processes with an additional resistance of a disinclination to let himself go. It is not certain whether the resistance is directed against the very act of day-dreaming itself or whether it is directed against the experimenter's ascertaining by means of tones what the day-dream concerns.

The negativistic individual reacts under greater control during crystal gazing than in the other periods, in order to make doubly sure that no stimuli interrupt his thoughts unawares. If he actually indulges in day-dreaming the array of resistances and inhibitions stand guard so to speak, permitting only quite intense stimuli

to arouse the dreamer. A crude analogy might help to clear up these points.

The response of the suggestible person may be compared to that of a man who retires with all the doors and windows open, while the reaction of the negativistic individual may be compared to the man who retires with all the windows locked and the doors barred.

It seems pertinent to close this account by raising the question as to whether the phenomena of lowered and raised thresholds is due to a change in sensory acuity or to a mental set or attitude. Tentatively it seems that the weight of the evidence is in favor of the latter hypothesis. Psychologists agree that attention increases skill in a conscious, difficult process, while it decreases skill in a process that could take place automatically. In the experiment the direction of attention of all the subjects before and after crystal gazing was on the stimulus, while the attention of the suggestible person during crystal gazing was on the day-dream. Now if the responses of the suggestible observer during this day-dreaming were automatic, as we tried to point out previously, then they should have been made with greater facility and ease. Such was found to be the case, as evidenced by the lowered threshold and certain obliviousness to responding. Thus it seems in the case of the suggestible person at least that a lowered threshold is due to a central neural set.

It also seems that the raised threshold of the negativistic observer is due to a central factor. It is difficult to conceive how resistances and inhibitions which are the important factors in raising the threshold could be due to a decrease in sensory acuity. The set or attitude of the observer seems to be a much more adequate explanation. The evidence, however, at hand is not sufficient to warrant a final answer on this point. What is needed is an experimentation carried on in other sensory fields, and such experiments are now in progress.

CONCLUSIONS

The data seem to warrant the drawing of the following conclusions:

1. That all individuals, normal and abnormal, can be arranged in the form of a straight line distribution with the extreme negativistic at one end and the extreme suggestible at the other.
2. That those individuals whose thresholds lower during crystal gazing are suggestible.
3. That those individuals whose thresholds rise during crystal gazing are negativistic.

4. The measurement of suggestibility or negativism can be obtained by ascertaining how much the threshold lowers or rises during periods of crystal gazing.
5. That the schizophrenic personality deviates from the normal in an unusual degree with a raised threshold.
6. That the hysteric personality deviates from the normal in an unusual degree with a lowered threshold.
7. That the lowering or raising of the threshold during crystal gazing is a function of suggestibility or of negativism, is demonstrated by the desire to enter into the abstraction on the part of those whose thresholds go down and the resistance to the abstraction by those whose thresholds go up.
8. That the direction of change in the threshold during crystal gazing can be used as an aid in the diagnosis of cases of schizophrenia and psychoneurosis.

NOTE ON THE RELATION BETWEEN GROUP TEST SCORE AND CAPACITY

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IF one man wishes to employ another it is possible to make inquiry among his friends, and when he learns of a desirable person whose services are available, such an one may be engaged with a fair degree of assurance. This seems to be the simplest method of hiring, and is essentially personal, but with larger concerns, employing an ever-changing multitude of men or women, this is not possible. Such a concern makes it known that vacancies exist, and the candidates present themselves. Presumably the candidate wishes to investigate the job; that is, he wishes to know the character of the concern, the working conditions, opportunity and pay, while the employer wishes to find out what kind of a person the candidate is and whether he is suitable for the particular work to be done. So, for ages past, good minds have been trying to improve the technique of hiring.

Most of these attempts have been directed along two lines: first, the reference; second, the interview. I have had no experience with the former, but from what I can learn, the reference is not considered very reliable. Candidates do not give as references those who will report adversely. A man just discharged from a concern for stealing and giving that concern as a reference surely needs a mental examination. Yet, on the theory that something is better than nothing, such references are widely used. This leaves, then, the interview as the principal factor in determining the candidate's fitness. Complaints of large turn-over, probation periods and other methods used seem to show a considerable degree of error in selecting.

The interview consists of two parts—personal history and personal examination—although the history is often written in the application. Furthermore, the history from the candidate shares many of the weaknesses of the reference, being largely under the control of the candidate and but slightly under the control of the interviewer. My own [1] personal study of many schemes of hiring, as well as the testimony of various employers, bears this out. Selecting the desirable applicant consists, in the main, of one man sitting on one side of a desk, another on the other side, and the

[1] Stearns, A. W., "The Classification of Naval Recruits," California State Journal of Medicine, April, 1919.

first guessing whether or not he wishes the second as an employee.

From observation I believe that intuitive judgment of mankind forms one of the principal assets of the successful man, and I have no doubt if such men could give their time to this work selection would be very accurate, but as a matter of fact they do not have the time. They delegate this work to a manager, who in turn delegates it to an employment manager, and again he often has interviewers who actually do the work. This, too, then is essentially personal and a matter of chance. The failure of this final product of effort along this line is attested by turn-over statistics everywhere.

Of late three groups of persons entirely outside of commercial life have given attention to this matter. The first (I hardly know how to designate them) have resurrected something of phrenology and physiognomy, have advertised widely, and the credulity of the business man is evidenced by their vogue. I know nothing of their results but regard their work as akin to fortune-telling, and foreign to science.

Next, the psychiatrists, with their vast experience with failures and conduct disorders, have learned the significance of certain careers and have developed skill in detecting the particular type of individual with whom they come in contact. As yet, most of their contributions have come from the clinic and consist of the enumeration of industrial failures of their psychopathic patients.

Lastly, the psychologists, recently armed with the group test developed through the exigencies of the war, have entered the field. Their work has been quantitative, in the sense that they estimate the capacity of the individual, and qualitative in that they try to find special ability or aptitude. I am not unmindful of the excellent work of physiologists, but their work has had less to do with mental factors.

Having had a somewhat extensive opportunity for studying the application of psychiatry and group testing in the naval service, I naturally wished a chance to study the application of similar methods to commercial or industrial life. Such an opportunity was presented at the R. H. White department store, through the interest of the president, Mr. George B. Johnson.

For a period of about three months each accepted candidate was interviewed along psychiatric lines, and each week those who had been employed during the previous week were given a group test. The test and the technique of the interview have been described elsewhere. Suffice it to say that the interview depended

upon the interpretation of appearance, geographical factors, former education, previous occupation and the general health of the individual. The group test consisted in the score arranged from the following five tests:

1. Trabue C
2. Dissected sentences from Binet-Simon Scale
3. Cancellation test
4. Memory span for numerals
5. Healy Code

Six months later, through the kindness of Mr. McFarland, employment manager, these individuals were graded according to the estimate of their serviceability to the concern.[1] The study concerns one hundred and twenty-three individuals, this being the run of the new employees.

Table 1 shows the comparative results of the two systems of marking.

TABLE I. NUMBER OF EACH PSYCHOLOGICAL GROUP IN EACH STORE CLASS [1]

	A	B	C	D	Total
4	5	8	8	4	25
3	4	13	7	5	29
2	1	7	7	3	18
1	4	25	17	5	51
Total	14	53	39	17	123

[1] For fuller explanation of the grading upon the basis of both the psychological tests and employers' ratings, see page 373. This information is necessary for interpreting the tables.

TABLE II. COMBINED RESULTS OF HIGHEST TWO AND LOWEST TWO PSYCHOLOGICAL AND STORE GROUPS

	A and B	C and D	
4 and 3	30	24	54
2 and 1	37	32	69
	67	56	123

TABLE III. PERCENTAGE OF EACH PSYCHOLOGICAL GROUP IN EACH STORE CLASS

	A	B	C	D	
4	20	32	32	16	100
3	13.8	44.8	27.6	17.2	103.4
2	5.5	38.7	38.7	16.6	99.5
1	7.8	49	33.3	9.8	99.9
	47.1	164.5	131.6	59.6	402.8

The marks on the test divide the subjects, roughly, into four quarters, as follows:

4 = superior
3 = high average
2 = low average
1 = inferior

The store marks are:

a = very good
b = good
c = fair
d = poor

It will be observed that there was little, if any, correlation between score of the test and store rating. Dividing the subjects into two store groups and two psychological groups yields no better results. (See Table II.)

Tables III and IV represent the same material in percentages. Analysis of the cards resulting from interviews showed twenty-

TABLE IV. PERCENTAGE OF EACH STORE CLASS IN EACH PSYCHOLOGICAL GROUP

	A	B	C	D	
4	35.7	15.1	20.5	23.5	94.8
3	28.5	24.5	17.9	29.4	100.3
2	7.1	13.2	17.9	17.6	55.8
1	28.5	47.1	43.5	29.4	148.5
	99.8	99.9	99.8	99.9	399.4

TABLE V. DISTRIBUTION OF POSSIBLE NERVOUS DISEASE CASES BY PSYCHOLOGICAL GROUP AND STORE CLASS

	A	B	C	D	
4		1	2	2	5
3		4	2	1	7
2		0	0	2	2
1		3	3	2	8
		8	7	7	22

two individuals who gave evidence in their answers of poor health, suggesting functional nervous disease.

Table V shows their place in the store and test ratings. This latter is of interest in connection with the findings of Dr. A. B. Emmons, 2nd,[2] who reports functional nervous disorder as one of the important causes of absenteeism in the department stores which he studied.

In so far as one is able to judge from so brief a study and so few cases, it would appear that intelligence as measured by the group test was not of vital importance in determining the success or failure of department store employees, and for this reason, tests, as an aid in the selection of employees, would not seem feasible; secondly, functional nervous disease appears to be an important element in failure, is relatively easy to detect, and special effort might well be made along these lines in history. There are indications that psychiatric knowledge can be of use in improving the technique of the interview.

[2] Emmonds, Arthur B., "Harvard Mercantile Health Work," Harvard Alumni Bulletin, March 8, 1922.

THE EFFECTS OF PRIMOGENITURE ON INTELLECTUAL CAPACITY[1]

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CERTAIN workers in the field of mental deficiency have contended that primogeniture is a contributory factor in the production of that condition. Attention seems to have been drawn to this fact by the discovery that a large percentage of institutional cases were first-born children. The force of this evidence in support of a theory of the causal nature of primogeniture was seriously weakened, however, by the further reflection that the number of first-born children in any total population is considerably in excess of the number of second and third born.

The writer has carried on psychometric investigations in the Alex Taylor School, Edmonton, Canada, for over six years. During that time he has tested almost two thousand children by the Stanford revision of the Binet scale. There are, of course, no low-grade defectives in the school population, but sufficient evidence has accumulated to warrant a tentative conclusion as to the effect of primogeniture on higher grade defectives, dull, and normal children. If mental defectives differ only in degree, and not in kind, from the other groups, then any conclusion applicable to the latter may, unless contrary evidence is provided, be considered applicable to the former.

The basis of comparison used in this survey is the I.Q. The ratings of 219 pairs of children are given, each pair being, respectively, the first and second children in a family. It has not seemed advisable to divide these children into the commonly accepted subdivisions of dull, normal, superior, and so on; the number of cases in each group would then be so small as to render generalization dangerous. The ratings obtained by the two groups of children are set forth in the following tables:

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[1] Prepared under the supervision of Professor E. D. MacPhee, Department of Psychology, University of Alberta.

I.Q.'s of First and Second Children in the Same Family

1st	2d	1st	2d	1st	2d	1st	2d	1st	2d	1st	2d
96	97	81	107	88	101	122	128	106	99	100	89
88	101	80	77	91	98	105	92	87	104	102	121
81	92	105	114	71	73	129	98	101	79	79	91
90	92	89	95	93	90	77	66	86	84	102	94
95	84	110	110	100	89	98	88	109	78	96	98
96	93	115	98	114	118	105	99	101	114	91	85
80	95	107	97	105	101	109	106	105	81	98	98
105	119	50	79	100	107	89	109	77	101	99	115
88	98	89	97	97	109	76	93	79	95	100	111
105	86	99	112	106	102	93	110	92	100	60	73
102	99	74	70	95	110	100	91	117	70	84	104
102	113	95	112	99	90	109	100	116	116	113	131
88	96	132	109	73	87	98	99	118	128	111	122
114	120	107	114	90	119	90	85	85	85	101	125
106	120	100	118	71	106	102	111	116	106	77	93
112	110	90	83	93	98	73	87	61	64	121	105
58	71	83	105	84	90	89	97	146	170	82	113
86	98	106	119	115	103	82	88	112	98	83	101
101	98	121	99	115	112	67	91	97	101	103	109
109	110	128	119	100	81	104	89	67	70	115	118
104	111	99	88	90	110	89	97	102	110	89	95
105	89	113	110	97	85	102	96	112	98	106	117
93	86	108	113	97	142	81	85	95	116	105	101
81	79	77	104	108	122	102	80	109	105	114	100
100	103	98	106	72	78	123	84	89	98	94	88
104	104	108	84	107	98	91	105	135	101	98	117
96	115	105	110	69	112	91	81	105	103	88	101
82	100	140	152	129	111	110	87	98	99	85	79
60	89	118	115	100	106	114	55	66	101	112	95
108	88	99	91	132	102	88	102	120	120	106	82
103	118	84	107	103	112	72	100	91	89	122	117
108	87	90	104	81	93	94	99	98	103	101	107
83	96	102	94	112	107	83	99	81	79	110	98
115	96	66	79	90	90	114	97	97	98	102	102
99	92	93	120	76	110	87	80	86	93	120	102
115	109	95	97	73	87	99	110	94	107	77	74
75	100	87	98	96	109						

The median difference in favor of the second child is + 4.5. The reliability of this median may be calculated from the formula[2]

$$1\tfrac{1}{4}\,\frac{\sqrt{\frac{\Sigma x^2}{N} - (\Sigma_x)^2}}{\sqrt{N}}$$

This deviation amounts to 1.16. The median difference should then be expressed as + 4.5 ± 1.16. Therefore the probability is even that the median difference in I.Q. between the first and second children lies between + 3.34 and + 5.66. It is almost certain (9997 chances out of 10,000) that it lies between + 1.02 and + 7.98.

[2] McCall. How to Measure in Education, p. 4002.

The writer recognizes that a large number of factors must be taken into account in explaining the differences in intellectual capacity of siblings. It is not our purpose to attempt an analysis of these factors, or to estimate the relative significance of primogeniture. The reliability of the distribution quoted above is very high and we may safely assume that primogeniture is the only variable at work in our results.

If this position be warranted, then the following conclusions are warranted.

1. First born children are, on the average, slightly lower in intellectual ability than second born children.
2. The extent of the difference is so small as to indicate that the part played by primogeniture is real, but relatively small.
3. The higher incidence of mental deficiency among first born children may be due in part, to the fact that they are the first born children in families in which the rest of the children are properly classified only as "borderline" or "backward" cases.

The writer has collected some data which suggest that there is a slight difference, in backward families at any rate, between second and third children but that this variation does not appear between subsequently-born siblings. Further experimentation will be necessary to test this hypothesis.

TWO HUNDRED CASES OF DEMENTIA PRECOX TESTED BY THE STANFORD REVISION

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DURING the months July-September, 1923, the Stanford revision of the Binet-Simon tests was given to 200 cases of dementia precox in the State Hospital for the Insane at Taunton, Massachusetts. The cases comprised men and women of all types, of all ages above sixteen, there being no special selection except of patients who were willing to coöperate. The tests were all given by the writer.

Some interesting data resulted. The mental ages attained range all the way from two to seventeen, as follows:

Mental age	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17
No. of cases	5	2	6	5	14	21	36	29	22	18	15	5	8	7	3	4

It will be observed that 140 cases are below the mental age of 11, that 108 cases are grouped about the mental ages of 7-8-9-10, 87 of whom are between 8 and 10. Only 27 cases are above the mental age of 12, 7 of whom attain the adult or superior adult standing by the grading of the scale.

It may be thought that this marked departure from what is considered to be average adult mental age (considering adult to be 13-14) is due to deterioration, which the very name dementia precox connotes, and which has been generally conceded to be a characteristic outcome of that disease. But except for the cases of marked deterioration, those who have regressed into an infantile stage—and these were in the very small minority of our cases—there is much evidence that an intellectual deterioration does not necessarily obtain, and is not constant along all lines. The fact that there is no real correlation between length of duration of the disease and amount of deterioration only strengthens our conclusions.

The cases may be divided into three groups: (a) Those cases whose deterioration to a low level is evident along all lines, and whose normal mental age it would be impossible to determine; (b) those cases who show definite blocking of thought processes, who have to be led by slow steps out of their own world into the world of reality, whose responses are uneven in quality and in accuracy, definitely affecting the mental age; (c) those cases whose answers are clear and definite, with no apparent blocking, and

whose mental ages seem to represent their normal mental ages so far as one can judge.

The reactions of each type to the questions of the test are characteristic. Of group (a) little need be said. They retain only definite concrete things, the type and number of such memories progressively diminishing in quality as they sink lower to the infantile stage. Some can do little more than the imitations of year two, give their name and sex, seldom their age (they come much nearer to their mental age than to their chronological age, in some instances hitting it exactly). Types of questions generally passed by these patients, according to the amount of deterioration shown, are the following: Pointing out objects in a picture (year 2), imitation (year 2), simple commands (year 2), pointing out features (year 3), naming familiar objects (year 3), sex and name (year 3), counting pennies (years 4 and 6), coins (year 6), fingers and bowknot (year 7), counting 20-1 (year 8), months and occasionally change and stamps (year 9). Type of questions most often failed by these deteriorated cases are those which demand any real thinking or discrimination of judgment or reasoning, such as comprehension above year 4, aesthetic comparison (year 5), definitions and patience (year 5), mutilated pictures (year 6), description of pictures and differences (year 7), similarities (year 8). Attention is not long sustained, hence such patients fall down on such memory tests as repetition of sentences, though they generally do better on digits. The higher types of this class repeat digits well above their mental age.

There is no possibility of determining the normal intellectual development of these patients except from their records, which vary much. In some instances we read, "He never did well in school"; in others, "She was always bright in school." In all cases of this type examined the psychosis developed early in life, and the deterioration seems to have been progressive and rather rapid.

The second group is very interesting. Much can be learned from them about the working of the human mind, for they have woven about themselves a mental web of their own making; and if the right thread be found the examiner can see how the pattern was originally made. The barrier of convention which keeps one human mind from revealing itself completely to another human mind by many conscious subterfuges does not exist in this class of patients, and given a chance they often tell so much that it is quite as easy to judge them as if they were normal, perhaps more so.

With this class of patients memory for digits forward is pecu-

liarly intact. It seems increased in some cases. No matter how slow their response, they give no impression of any difficulty here. It is exactly as if the numbers registered on a metallic surface and all they had to do was to read them off. One patient gained 20 months in his mental age by repeating all digits forward to year 16 and all backward to year 18, although he failed on every other test above year 9, except the vocabulary, year 10. As a general rule, however, digits backward correlate better with mental age than digits forward.

The same thing holds true with questions involving arithmetical computation, which process is retained by this group longer than other processes. So also with other questions of a definite concrete nature. Questions most often passed above mental age are the following: Counting 20-1 (year 8), change, months, and stamps (year 9), form board (year 10), induction, arithmetical reasoning, and clock (year 14), enclosed boxes (year 16). Questions most often failed are the weights (year 9), absurdities, designs, and comprehension (year 10), abstract words, fables, picture interpretation, and similarities (year 12).

It will be noted that the tests failed by patients of the second class resemble in quality those failed by patients of the first class, except that in the former case answers entirely unconventional, abnormal, or distorted will often be given, while in the case of the latter silence or "I don't know" is more often the response.

The two tests which best give us a glimpse into the minds of this type of dementia precox are the vocabulary and the fable interpretations. Interpretations of the fables are generally distorted or personal or bizarre, while in the vocabulary there is much unevenness, patient answering accurately for a while, then becoming dissociated and giving erratic replies. He seems to come from his unreal world, then retire into it without notice, and the examiner must be on the alert to detect this. A change in the type of question generally serves the purpose. Two or three changes of this sort are often necessary if a full test is to be given and a fair estimate obtained. Examples of this type call for all the tact and patience of which one is capable. One case of a man who had not talked for months illustrates this point. After thirty-five minutes the examiner persuaded him to write his responses, which were well and quickly done. When that type of question had been exhausted, the examiner frankly told the patient that she knew he hated to talk, that she could not find any more questions to which he could write the answer, that she would more than appreciate his saying what he thought. After trying in vain for some time

he began to talk when he saw the pictures. The ice was broken, and he had no further difficulty. The whole examination took nearly two hours, but it convinced the examiner that what looks like stupid apathy or indifference is sometimes mere habit, a compensatory reaction which works well, and that much is behind that seemingly impenetrable mask.

The mental age of type (b) is an inaccurate representation of their intelligence. The questions answered are irregular, and one can see deterioration from a higher level by the type of vocabulary definition given, and by the fact that invariably some difficult questions on a higher level are always answered. From the nature of the questions answered above the mental age level (whether involving mere memory, or reasoning and judgment), one can feel justified in concluding how much deterioration has taken place. There are many varieties of this type, as of all types—some being peculiarly the linguistic, impractical type, others showing rather good practical or manual skill. On the whole the linguistic type predominates.

So far as type (c) is concerned, their reactions seemed normal, and one feels justified in giving mental age attained as not far from their normal mental age. Of the 200 cases tested, by far the majority come from this class, and the more cases one tests the more justification there seems to be for the conclusion that there is not necessarily an intellectual deterioration in dementia precox cases; that many of these cases were of the low normal or high moron type to begin with; that a psychosis may develop at any age level above the low grade imbecile stage; that in most cases of class (c) an innate inability to compete successfully in the realm of conventional social adjustment was the cause of the psychosis, called by the rather inadequate term dementia precox. In the hospital they make their adjustment according to their personality make-up, and they retain a large amount of their intellectual equipment. There are several cases of dementia precox remission who have left the hospital and are succeeding outside. Many men and some women in the hospital are given definite set tasks of a regular routine nature, and find their places in the work of the institution, showing themselves capable and trustworthy, even though many remain deluded and hallucinated. It may be significant to note that a large majority of patients classified as paranoid dementia precox were found in type (c), while in types (a) and (b) we find more of those who were diagnosed hebephrenic or catatonic.

It will be seen that the questions which give the least difficulty to all these patients are those involving remote memory and habit-

ual use over a long period of time, and those which apply to a practical concrete type of thing like the form board, or copying a square or diamond. The arrangement of the five weights, involving as it does a rather fine discrimination of a motor type, is often failed. This seems to be the case with normal adults, however, so far as the writer has observed. The induction test (year 14) is often passed by a subject who fails in all the 12-year tests. The same thing holds true of problems of fact (year 14), with which tests they find much less difficulty than with the interpretation of the fables. The reason for the rather common failure to pass the enclosed boxes test of year 16 is not the intrinsic difficulty of the test, but the fact that the majority of the subjects tend to answer impulsively or carelessly. The same thing holds true in the case of the second of the arithmetic problems (year 14).

The questions which have the greatest difficulty for the cases of dementia precox are those which have to do with ideational judgment, with the ability to generalize, to understand motives underlying social acts. The best single test of this sort is the interpretation of the fables—for those who reach at least the mental age of 10. It is of marked significance that only 26 patients of our whole group obtained the 12-year score in the fable tests, and only 9 of this number obtained the 16-year-old score, although 128 patients were given the test, 59 of whom answer questions above year 12.

The same thing holds true with patients on a lower level, where we find the same judgment defect. Absurdities were rarely grasped. The question, "Why judge a person by his acts rather than by his words?" was answered from an abnormal bias. On very low levels, as we have seen, everything which deals with ideas seems to be beyond their comprehension, all such questions being very difficult.

The contention, therefore, that it is a volitional and affective disorder, rather than an intellectual defect, which is characteristic of dementia precox, seems to be amply demonstrated from the results of these tests. It is a curious and a tragic thing to watch a man or woman of natively high intellectual capacity answer with ease other difficult questions of the adult group, and then fall flat in his judgment of an ideational type, giving a childish or a distorted interpretation of a fable whose moral should be easy for a man of his caliber to grasp. This reaction seems to resemble a hole burned into a piece of cloth otherwise intact. It can be patched and made useful, but can never be fully whole again.

From what has been said above it will be seen that it is not safe

to stop at the year level where all tests are failed. Some of these tests are easier for adults than for children for whom the tests were made. There are certain questions which should be given to every adult, unless he is of very low-grade ability. We refer to the induction test (year 14), problems of fact and the clock test (year 14), paper cutting (year 18), and abstract words and enclosed boxes (year 16). We have several instances where patients in class (b), and also in class (c), passed some question of years 14 or 16 when they failed on all in year 12. It will be observed that several of the questions of year 12 tend to emphasize judgment of an ideational sort, while those mentioned above are of a more definite concrete sort.

There is no doubt but that it is unfair to insist upon a strict observance of the time element in cases of dementia precox where the thought processes are slowed up. Since we are testing intelligence rather than speed, and since their normal rate of speed is much retarded, it seems fair to grade answers plus if they are answered within a reasonable time. This has not been done with these cases, but overtime was more than once the sole cause for failure, and in the opinion of the examiner an unfair one. Tests most affected by this retardation are counting 20–1 in forty seconds, naming months in fifteen seconds, arithmetical reasoning in one minute, and ingenuity tests in five minutes each. In cases where time was not a factor, the examiner found that patience was indeed a virtue, and that an accurate answer came long after the question was put to the patient. The process was much like waiting for a long-distance telephone call to be connected when the line was busy. The answer was worth the waiting for, when it did come.

One word with regard to the subjective element entering into the giving of the tests. It seems to be larger than is generally supposed. The chance of misunderstanding the questions on the part of the subject is greater than it might appear on the surface, not only for adults, but also for children, and especially for the abnormal, whose attention is easily diverted, and who have to be treated individually in every instance. If we do not get a full performance we are unfair—and we must "put it across" to the subject. Tact and diplomacy, patience and understanding of any emotional complication, are essential. With the abnormal this is of course very marked. Two sittings must sometimes be made. As a rule, questions must be given very slowly, especially such questions as reading the fables, where too much speed in reading precludes the

possibility of understanding on the part of patients whose processes are all retarded.

In the article by Wells and Kelley, "Intelligence and Psychosis," in the *American Journal of Insanity*, July, 1920, 19 out of 22 cases of dementia precox tested by the Stanford scale in McLean Hospital, Waverley, Massachusetts, had a mental age of 11 or more. Only 60 out of our 200 cases attained as high a mental age as 11. This difference is probably due to the fact that our group is larger, and more variation is to be expected. By far the larger number of our cases come from the factory class or from the class of unskilled mechanics. Education beyond the grammar school is the exception, not the rule, though a dentist, a lawyer, and a few teachers figure conspicuously on our list. The conclusions which were made in that paper are confirmed here, that dementia precox involves primarily a judgment and volitional defect rather than an intellectual one, and we feel satisfied to affirm that in a large majority of our cases the mental age attained is close to what was the normal mental age. Nor could we call them all feebleminded. There were some feebleminded among them, but the majority represented that low-average type who had been able to support themselves. The upper limits of feeblemindedness and the lower limits of normality do not by any means coincide, hence a mental age of eight or nine in an adult may or may not represent feeblemindedness, depending upon other things than a psychological test. Among our cases it is significant that the few patients of the higher mental ages showed as marked a judgment defect as those who graded lower, and it is because of this defect that their psychosis developed. Dementia precox is found among all classes intellectually.

The question, then, is not of intellect in the narrower sense of the term. The linguistic type, the scholarly type, the manual type, are all found among our cases. There seems much probability that many of these patients could have been saved to the constructive work of the world if, through individual study and treatment at an early stage of the disease, an adjustment could have been made where the odds against them would not have been too great. One is impressed with the large amount of physical and of intellectual capacity in most of the dementia precox cases, and the glimpse we occasionally get behind the curtain where they so cleverly conceal themselves makes us believe that this energy which is breaking up the individual might be harnessed to something really worth while, could the connection only be made early enough.

A STUDY OF KRAEPELIN'S CONTINUOUS-SUBTRACTION TEST

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KRAEPELIN'S continuous-subtraction test is ordinarily used as a test for the determination of the ability of adults to control attention over a short period of time. It is also frequently employed for the same purpose in the clinical examination of children.

In his "Clinical Psychiatry,"[1] Kraepelin states that "the disturbances of attention can usually be determined in a satisfactory manner by the use of the progressive adding and subtracting test, such as subtracting seven successfully from one hundred down to zero." He apparently made no attempt to standardize this test, or to make it an accurate means of objective measurement of attention. He gives no definite way of scoring by means of which various examiners may agree as to the degree of distractibility in a patient.

In order to determine the reliability of the test as a measure of sustained attention in children, it was given to 113 unselected cases referred for psychological examination.[2] Each child was given in addition a Stanford-Binet intelligence examination. The chronological age of the subjects ranged from seven to sixteen years, mental age from seven to sixteen, and the intelligence quotient from 50 to 120.

In applying the test, the subject was told to begin with 100 and subtract 7, and to keep subtracting 7 until nothing remained. The time was taken, and the number of errors noted. An error consisted of any arithmetical miscalculation. Subtracting by ten was considered a total failure, as well as any apparent repetition of similar numbers throughout the series, without any actual subtraction, *e.g.*, 100, 93, 85, 73, 65, 53, 45, 33, etc.

Tables I to III indicate that the percentage of total failures was related to all three factors of chronological age, mental age and intelligence quotient, but most consistently to mental age and intelligence quotient. As to chronological age, the percentage of

[1] Clinical Psychiatry (Translated by A. Ross Diefendorf, 1907) adapted and abstracted from the 7th German Edition of Kraepelin's "Lehrbuch der Psychiatrie". The Macmillan Co., 1907.

[2] The data were obtained for the most part at the Institute for Juvenile Research, Chicago, Illinois.

failures decreased steadily with the increase in chronological age through thirteen, but from this point on varied irregularly. There was no specific tendency for the average number of errors to decrease with increase in chronological age or intelligence quotient until a chronological age of sixteen and an I.Q. of 90 were reached, so that the number of errors cannot be regarded as a correlate

TABLE I. RESULTS ACCORDING TO CHRONOLOGICAL AGE.

Age	Complete Failures		Successes					Total number of cases
	No.	%	Av. no. of errors	Range of errors	Av. time	No.	%	
7	1	100.0						1
8	0	0.0						0
9	5	100.0						5
10	7	63.6	4.0	0–9	2′ 3″	4	36.4	11
11	7	43.8	5.0	0–8	2′14″	9	56.2	16
12	3	20.0	3.4	0–9	2′42″	12	80.0	15
13	1	5.0	3.4	0–8	2′19″	19	95.0	20
14	3	16.7	4.4	0–9	2′15″	15	83.3	18
15	4	28.6	3.8	0–8	2′33″	10	71.4	14
16	2	15.4	2.2	0–7	1′33″	11	84.6	13
Total	33					80		113

TABLE II. RESULTS ACCORDING TO MENTAL AGE.

Age	Complete failures		Successes					Total number of cases
	No.	%	Av. no. of errors	Range of errors	Av. time	No.	%	
7	2	100.0						2
8	15	78.9	6.3	3–9	2′20″	4	21.1	9
9	8	44.4	4.7	0–7	2′59″	10	55.6	18
10	4	16.7	5.1	0–9	2′28″	20	83.3	24
11	2	13.3	4.4	1–8	2′ 7″	13	86.7	15
12	2	14.3	3.3	0–8	1′50″	12	85.7	14
13	0		3.5	0–9	1′57″	10	100.0	10
14	0		2.5	0–3	1′13″	7	100.0	7
15	0		2.0		4′21″	1	100.0	1
16	0		0.7	0–2	51″	3	100.0	3
Total	33					80		113

either of chronological age or intelligence quotient. There was, however, a fairly regular decrease in the average number of errors with increase in mental age, so that with a large enough number of cases, it might be possible to establish by means of this test the mental age level according to the number of errors made. The range of errors varied irregularly, showing a definite tendency to decrease only in the upper mental age levels. The average time of performance showed the same irregular variation, with no relation to intelligence quotient, but a dropping off abruptly at chronological and mental ages of sixteen.

Table IV indicates that the time of performance ranged from one-half minute to seven minutes, with errors ranging from zero

TABLE III. RESULTS ACCORDING TO INTELLIGENCE QUOTIENT.

I.Q.	Complete failures		Successes					Total number of cases
	No.	%	Av. no. of errors	Range of errors	Av. time	No.	%	
50-59	3	50.0	6.0	5–8	1′59″	3	50.0	6
60-69	3	50.0	4.0	1–7	1′48″	3	50.0	6
70-79	12	33.3	4.7	0–8	3′ 3″	24	66.7	36
80-89	9	29.0	5.1	0–9	2′12″	22	71.0	31
90-99	4	23.5	2.8	0–9	1′ 3″	13	76.5	17
100+	2	11.8	1.8	0–6	1′16″	15	88.2	17
Total	33					80		113

to nine. The median number of errors was four, the median time one minute and fifty-two seconds. The time of performance of the majority of the cases was three minutes or less. The correlation of .401 indicates a fairly definite relation between time and the number of errors made, the errors, on the whole, increasing with an increased time of performance. From the table it is seen, however, that this was particularly the case only after a time of four and one-half minutes was exceeded.

CONCLUSION

The continuous-subtraction test, for children, is largely a reasoning test rather than an attention test, requiring a certain degree of mental maturity before it can be accomplished. With the increase in mental development, the ability to complete the test increases proportionately; likewise, the higher the intelligence

quotient, the greater the probabilities are that a subject may perform the test. The number of errors made, however, is related directly only to mental age, and not to chronological age or intelligence quotient, unless the subject is sixteen years of age or older, and has an intelligence quotient of 90 or above, *i.e.*, adequate intelligence, in which case the minimum number of errors may be expected. Similarly, the average time of performance is highly

TABLE IV. DISTRIBUTION OF TIME AND ERRORS

Time in Seconds	Errors										Total
	0	1	2	3	4	5	6	7	8	9	
31– 60	5	3	2	...	...	...	...	...	1	...	11
61– 90	4	1	2	3	1	1	...	...	1	...	13
91–120	1	1	4	1	7	3	3	1	1	...	22
121–150	...	...	2	...	...	1	1	1	...	...	5
151–180	1	...	1	3	...	1	1	...	2	2	11
181–210	1	...	1	...	...	1	...	...	...	...	3
211–240	...	...	...	1	2	2	...	1	1	...	7
241–270	1	...	1	...	1	...	...	...	...	...	3
271–300	...	...	...	...	...	...	...	...	...	1	1
301–330	...	...	...	...	...	2	...	...	...	...	2
331–360	...	...	...	...	...	...	...	1	...	...	1
361–390	...	...	...	...	...	...	...	...	...	...	...
391–420	...	...	...	...	...	...	...	1	...	...	1
Total	13	5	13	8	11	11	5	5	6	3	80

Correlation .401 P. E. .063

variable and shows no definite decrease toward a low level until a chronological and mental age of sixteen are reached. In other words, only in the case of the normal adult is the test applicable as an attention test. In more immature subjects, the difficulty of the test is such that the number of errors is increased, with the resulting increase in time of performance; in this way, the reasoning element of the test is emphasized to such an extent that the test cannot serve as an indicator of the ability to sustain attention over a short period of time.

The results indicate also that an adult with a mental age of sixteen should not make more than two errors and should perform the test within one minute. If these requirements are not fulfilled, it may then be assumed, and only then, that attention is not sustained.[3]

[3] The writer is indebted to Myrtle Raymaker Worthington for assistance in perparation of the data.

EXPERIMENTS IN WAKING HYPNOSIS FOR INSTRUCTIONAL PURPOSES[1]

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IN another article[2] I have discussed certain values of experiments in hypnosis for instructional purposes. In that article I called attention to three ways in which class experiments in hypnosis may aid instruction in general psychology, as follows: first, through the arousal of increased interest among students; second, through the increased effectiveness of instruction against telepathy, spiritism, and occultism in general; and third, through the increase of concreteness and clearness in presenting various fundamental psychological facts. Professor McDougall, in his article on "Hypnotism" in the *Encyclopaedia Britannica*,[3] says, "As a method of psychological investigation hypnotism has proved . . . capable of throwing much light on the constitution of the mind, has opened up a number of problems of the deepest interest, and has done more than any of the many branches of modern psychology to show the limitations and comparative barrenness of the old psychology that relied on introspection alone and figured as a department of general philosophy." If hypnotism as a field of research has accomplished results for psychology deserving of this high praise, some of the classical experiments deserve to become standardized for instructional purposes in classroom and laboratory in general as well as in abnormal psychology.

In the article referred to above, I did not describe in detail the nature of the experiments which I have found most useful; nor did I discuss the distinction which I regard as important between what I shall now call sleeping hypnosis and waking hypnosis. The purpose of the present paper is to justify the use of the term "waking hypnosis" and to describe in some detail the methods employed and results obtained in typical experiments.

I. THE MEANING OF WAKING HYPNOSIS

In 1843, with the publication of his book *Neurypnology*, Dr. James Braid, of Manchester, England, contributed to the scientific world much of the theory as well as the vocabulary of

[1] Presented in condensed form at the meeting of the American Psychological Association, University of Wisconsin, December 29, 1923.
[2] "Hypnosis in the Service of the Instructor", *Psych. Rev.*, 1924, Vol. 31, pp. 88-91.
[3] 11th edition, Vol. 14, p. 203.

modern hypnotism. He coined the term "hypnotism" (from the Greek, meaning sleep), and he defined it as "nervous sleep." (He did not distinguish "hypnotism" from "hypnosis," for he did not use the latter word.) In 1866 Dr. Liébeault, in France, published his book on hypnotism under the title *Du Sommeil;* and, as the title of the book is sufficient to indicate, he regarded hypnosis as a form of artificial sleep. Dr. Liébeault was the source of what is called the Nancy school of hypnotism, and "to him we owe the development of modern hypnotism";[4] though Dr. Bernheim, attracted by Dr. Liébeault's work in 1882, has been the most influential of any of the workers in the Nancy school. As with Braid and Liébeault, so at the present time the commonest type of definition of hypnosis is in terms of sleep. Thus Baldwin's *Dictionary of Philosophy and Psychology* defines hypnosis as "an artificially induced sleep-like or trance-like condition of mind and body." And Professor McDougall, in his article in the *Encyclopaedia Britannica,* calls hypnosis "a condition allied to normal sleep." He says further that although the Nancy school "went too far" in seeking to identify hypnosis with normal sleep, "the views of the Nancy investigators have prevailed, and are now in the main generally accepted."

That this is true so far as American psychologists are concerned is evident from reference to their use of the term "hypnosis." Some psychologists and physicians writing in America, for example, Münsterberg[5] and Coriat,[6] have criticized the sleep theory of hypnosis; and yet they have accepted it to some extent. Thus Münsterberg said, "It lies near to compare it [hypnosis] with sleep"[7]; and he further said[8] that suggesting sleep is the best method of inducing hypnosis. Coriat admits that "to a limited extent hypnosis outwardly resembles normal sleep. The hypnotic state," he says, "can be brought about by the same influence and conditions as produce sleep."[9] Sidis explained hypnosis in terms of dissociation without reference to sleep; and yet he seemed to assume that sleep was usually involved, for he continued to use the vocabulary and methods of sleeping hypnosis.[10] Prince, like Sidis, defines hypnosis entirely in terms of dissociation without

[4] J. Milne Bramwell, *Hypnotism*, 3d edition, p. 32.
[5] *Psychotherapy*, Ch. 5.
[6] *Abnormal Psychology*, Ch. 8.
[7] *Op. cit.*, p. 112.
[8] *Ibid.*, p. 114; *Psychology, General and Applied*, p. 258.
[9] *Op. cit.*, p. 198.
[10] *Psychology of Suggestion*, pp. 9, 13, 14, 119, 151.

reference to sleep;[11] but he, too, like Sidis, constantly speaks of it in terms of sleep, as a state from which the subject "wakes."[12] Though Scott in general speaks of hypnosis as "simply an extreme example of suggestion,"[13] he sometimes refers to it as "artificial sleep."[14]

Practically all the references to hypnosis in the American literature in general psychology are, without much if any qualification, in terms primarily of an artificially induced state of sleep. Thus Pillsbury says of it, "The state is much more readily induced if the patient remains passive and is told from time to time that he is going to sleep."[15] Judd says, "In many respects it is a condition closely related to normal sleep."[16] Angell says, "Hypnosis . . ., as the word suggests, is a state in many particulars closely resembling sleep."[17] Warren says, "The peculiar behavior of a hypnotized subject is understood if we compare him with a dreamer."[18] Breese says, "The first stages of hypnosis resemble sleep."[19] Seashore says, "There is . . . a kinship between the dream state and the hypnotic state."[20] Woodworth says, "Hypnosis is a sleeplike and passive state."[21] Titchener says, "Hypnosis may . . . be regarded as a state in which the organism is partly asleep, and partly awake."[22] James, writing in 1890, defined hypnosis in terms of sleep, and regarded "the simple verbal suggestion of sleep (the so-called Nancy method)"[23] as the best method of hypnotizing.

It should be added, also, that standard works on hypnosis by writers such as Moll, Forel, Lloyd Tuckey, and others, treat hypnosis primarily as a state similar to natural sleep, though noting, as we shall see, that in some cases of hypnosis sleep may

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[11] *The Unconscious*, p. 62.
[12] *Ibid.*, pp. 31, 38, 64, 66, 67, 73, 96. Since writing this article I have seen Dr. Prince's latest published statement on hypnosis (this journal, 1923, Vol. 18, pp. 238–43). Here Dr. Prince defines hypnosis in terms of "depersonalization" and "repersonalization", of dissociation and synthesis. He says, "The state may vary all the way from simple abstraction to extreme alterations of personality commonly termed secondary personality" (p. 238). But still Dr. Prince occasionally falls into the use of the ordinary vocabulary of sleeping hypnosis, and twice in the article he contrasts hypnosis with "the waking state" (pp. 240, 242).
[13] *Influencing Men in Business*, p. 62.
[14] *E.g.*, in *The Psychology of Advertising*, p. 174.
[15] *Fundamentals of Psychology*, Revised Edition, pp. 578, 79.
[16] *Psychology*, Revised Edition, p. 279.
[17] *Introduction to Psychology*, p. 253.
[18] *Elements of Human Psychology*, p. 322.
[19] *Psychology*, p. 451.
[20] *Introduction to Psychology*, p. 371.
[21] *Psychology*, p. 547.
[22] *A Beginner's Psychology*, p. 341.
[23] *Principles of Psychology*, Vol. 2, p. 593, note.

not be present. Even Bernheim, in spite of criticisms of the sleep theory, which we shall point out, did not actually break away from the influence of the theory, or, so far as he did, he wished to suppress the term "hypnosis" altogether.

Further reference to the past history of hypnotism, or to present-day theories as found in standard textbooks, is unnecessary to show that the general trend of opinion has been, and still is, to regard hypnosis as a state allied to natural sleep. This seems strange, however, especially in view of the *complete* history of the term and of *all* the observed facts from the time of Braid to the present. For, ever since the time of Braid, phenomena commonly called hypnotic have been repeatedly produced in the completely waking state.

In 1847 Braid attempted to suppress the term "hypnotism," having discovered that sleep is not essential to all the phenomena which he had previously called hypnotic.[24] He attempted to substitute the term "monoideism." If the term "hypnotism" were still to be used, Braid said, it should be restricted to those cases of artificial sleep which are followed by amnesia. In 1883 Bernheim reported to the Congress for the Advancement of Science, at Rouen, France, the production in the waking state of apparently all the phenomena usually called hypnotic except amnesia.[25] At that time Bernheim was unaware of Braid's observations of the production of hypnotic phenomena in the waking state, but shortly afterwards Bernheim came upon writings by Braid which showed that Braid had discovered the production in the waking state of paralysis, contractures, illusions, and hallucinations. Bernheim seems to have been the first to add analgesia and anesthesia to the list.[26] In 1892 a Committee of the British Medical Association reported that "sleep . . . is not necessarily present" in what is called hypnosis.[27] Since Bernheim's rediscovery of hypnotic phenomena in the waking state, numerous observers have confirmed and restated this fact, *e.g.*, Lloyd Tuckey, Moll, and Forel. Some have supposed that hypnotic phenomena in the waking state could be produced only in "trained subjects," previously hypnotized by the sleeping method. James, *e.g.*, expressed this opinion.[28] But Bernheim, Lloyd Tuckey, Moll, Forel, and others have observed and reported the production of hypnotic phenomena in the waking state *with subjects never*

[24] See Bramwell, *op. cit.*, p. 283; Bernheim, *Suggestive Therapeutics*, p. 86.
[25] *Op. cit.*, p. 85.
[26] *Ibid.*, p. 86.
[27] Bramwell, *op. cit.*, p. 36.
[28] *Op. cit.*, p. 615.

before hypnotized.[29] And finally, Bernheim has reported that amnesia is no exception to the rule that, as he has expressed it, *all* the phenomena commonly called hypnotic can be produced in the waking state.[30]

If *all* the phenomena commonly called hypnotic, including amnesia, can be produced in the completely waking state, in subjects who have never been hypnotized before, should we follow Bernheim's counsel and "suppress completely the word 'hypnosis' and replace it by 'the state of suggestion'!"[31] Or should we be determined more by practical convenience than by etymology, and say, with Moll,[32] "Hypnosis is not invariably a state of sleep"! The second alternative is clearly to be preferred. "Post-state-of-suggestion," *e.g.*, would be a cumbersome substitute for "post-hypnotic". And furthermore, to speak of "suggestion in the waking state" does not convey the meaning intended if one refers to the production of contractures, anesthesias, etc., even including amnesia and post-hypnotic phenomena. Sidis recognized the production in the waking state of a *few* hypnotic phenomena with a *few* subjects but called it rare,[33] and an exception to the general rule that the suggestibility of waking persons is of an opposite type to that of subjects in the hypnotic sleep. The general rule is, according to Sidis, that suggestion to waking subjects in order to be effective must be indirect and veiled; while to subjects in the hypnotic sleep effective suggestions may be direct. Sidis was certainly wrong in failing to observe that *direct* suggestion is effective in producing hypnotic phenomena in the waking state with practically all subjects; and yet, through usage, "suggestion in the waking state" does convey the meaning which Sidis ascribes to it, namely indirect suggestion, and not the production of the typical hypnotic phenomena.

The long and practically universal recognition given to amnesia (following the state) as a test of hypnosis—usually a test of the deepest stages, is evident to any student of the subject. Thus Braid, as we have seen, after attempting to suppress the term "hypnotism," or, as we would now say, "hypnosis," admitted it in cases where amnesia could be produced. "The earlier classifications such as those of Liébeault and Bernheim . . . made

[29] Bernheim, *De la Suggestion*, Paris, 1911, p. 16; Lloyd Tuckey, *Psychotherapeutics*, 3d edition, p. 69, note; Moll, *Hypnotism*, 4th edition, translated, p. 197; Forel, *Hypnotism*, 5th edition, translated, p. 117.

[30] *De la Suggestion*, p. 86.

[31] *Hypnotisme et Suggestion*, 3d edition, Paris, 1910, p. 77.

[32] *Op. cit.*, p. 47, note.

[33] *Op. cit.*, p. 27.

the criterion that they worked on, the loss of memory."[34] Sidis said, "Amnesia is the ripe fruit of hypnosis."[35] Coriat calls "loss of memory" one of the "four most important symptoms of hypnosis."[36] Pierre Janet says, "Hypnosis is just a state that leaves no remembrances with the patient when he awakes."[37] And Moll said, "We must call a state hypnosis . . . if there is a subsequent loss of memory."[38]

Since amnesia has received such long and general recognition as a test either of hypnosis in contrast with a non-hypnotic state, or of the deeper stages of hypnosis in contrast with the lighter stages, there is certainly sufficient justification for calling hypnotic a state in which amnesia can be produced, even though the state is a waking one. Such would be a state of waking hypnosis, the amnesia being produced by direct suggestion, and the waking condition being maintained normally in the absence of any suggestion of sleep. And having admitted the use of the term "waking hypnosis" in cases where amnesia is produced in the waking state, we have made the way clear for an extension of the term to those cases of contractures, anesthesias, and the like, suggested in the waking state even without the production of amnesia. However, subjects in whom amnesia cannot be produced in the waking state are not suitable for the experiments which I am going to describe, and consequently insistence upon this last point is not necessary.

Waking hypnosis is in no sense to be construed as a form necessarily of light or slight hypnosis, for in the production of amnesia and some of the other hypnotic and post-hypnotic phenomena it is like the usual deep hypnosis except for the absence of sleep. It is to be defined, not by contrast with deep hypnosis, but merely by contrast with sleeping hypnosis. It is to be called hypnosis for, inasmuch as the *essential* feature of sleeping hypnosis (as Sidis, Prince, McDougall, Coriat, and others, have shown) is dissociation, it would claim to be a more direct application of the principles of dissociation without reference to sleep at all. All sleep is dissociation, but not all dissociation is sleep. Ordinary lapses of memory in everyday life and absent-minded acts occurring during times of concentrated attention are cases of dissociation, but not of sleep. Just as exponents of sleeping hypnosis take their cue from dissociation as illustrated by sleep,

[34] H. C. Miller, *Hypnotism and Disease*, Boston, 1912, p. 70.
[35] *Op. cit.*, p. 71.
[36] *Op. cit.*, p. 197.
[37] *Mental States of Hystericals*, p. 96.
[38] *Op. cit.*, p. 197.

exponents of waking hypnosis would take their cue from such examples of dissociation in normal waking life as absent-minded acts or ordinary lapses of memory.

Comparison of waking hypnosis with what Sidis called the hypnoidal state may make the meaning of the former clearer. The two are directly opposite. Waking hypnosis is genuinely hypnotic and genuinely waking, while the hypnoidal state is a sort of sleep state, and is not hypnotic. Sidis said of it: "We must warn . . . against . . . confusion with any hypnosis, light or otherwise. The hypnoidal state . . . is widely different from hypnosis. The hypnoidal state . . . is a sleep state. . . . Sleep developed out of the hypnoidal state." [39]

The relationship between what I call waking hypnosis and the theories and practice of the "new Nancy school" (Coué and his disciple at Geneva, Baudouin) should be pointed out. There seems to be even *less* recognition of real waking hypnosis by Coué and Baudouin than by Bernheim, of the old Nancy school. Coué has largely abandoned sleeping hypnosis,[40] and his simple experiments in contractures for the purpose of teaching auto-suggestion "are carried out when the subject is in the waking state." [41] Coué himself denies that such experiments are hypnotic, apparently quibbling over the etymological significance of the term. Baudouin prefers to say that Coué's "method is now wholly based upon slight hypnosis." [42] But, Baudouin says, these "slighter degrees of hypnosis . . . are not followed by amnesia"; [43] and, in fact, Coué and Baudouin have apparently expressed no interest in the production of the typical phenomena of deep sleeping hypnosis with the subject in the waking state. Baudouin seems not to recognize that amnesia can be produced in the waking state, saying even that it can rarely be produced on the first occasion in sleeping hypnosis.[43a] The recognition by Bernheim that amnesia can be produced in the waking state seems to have been overlooked by the new Nancy school. And I might add that I have not found in Bernheim's writings any description of significant experiments involving amnesia in the waking state.

In the development of my technique, however, as I shall point out later, I have been influenced to some extent by the methods employed by Coué as described by Baudouin. These methods are

[39] *The Causation and Treatment of Psychopathic Diseases*, p. 363.
[40] Baudouin, *Suggestion and Autosuggestion*, p. 308.
[41] *Ibid.*, p. 262.
[42] *Ibid.*, p. 245.
[43] *Ibid.*, p. 296.
[43a] *Ibid.*, p. 244.

capable of elaboration and extension so as to produce the phenomena associated with deep sleeping hypnosis, such as amnesia and post-hypnotic phenomena, without the production of a sleeping or even a drowsy state.

A description such as I have attempted of waking hypnosis and its contrast with sleeping hypnosis can be given without raising the vexed question of just what constitutes sleep in its physiological and psychological aspects. The objection might be made that my method produces an hypnotic sleep state without such being suggested directly; and in the case of subjects previously put into the deep hypnotic trance this is true enough. For this reason I have confined my experiments in waking hypnosis to subjects never before hypnotized. Anyone who has seen the production of simple contractures such, for example, as those of the Coué type, by direct suggestion to subjects never before hypnotized and without any suggestion of sleep, has noted the entire absence of the characteristics of hypnotic sleep. And there are no more signs of sleep even when one goes further by similar methods and produces amnesia and post-hypnotic phenomena.

II. EXPERIMENTS IN WAKING HYPNOSIS

My first practical experience with hypnosis, especially in the effort to illustrate in a course in abnormal psychology points made by Dr. Prince in *The Unconscious*, was with the usual type of sleeping hypnosis. I soon found it possible and expedient, however, to use a completely waking type to illustrate the same points. Now, after having done experiments on several hundred subjects, in groups or individually, by means of the method of waking hypnosis, I have become further convinced of its merits.

The method of waking hypnosis possesses the following advantages, in my opinion, over the various methods of sleeping hypnosis, at least for class demonstration and instructional purposes, and in some cases for therapeutic purposes. First, it is less mysterious in appearance, and the total impression is more desirable. The psychologist who uses hypnosis partly for the purpose of teaching against occultism desires to avoid the appearance of an occult procedure. Second, it usually takes less time. With an individual subject or with a group, one usually begins to get results in two or three minutes, if not in five or ten seconds; while sleeping hypnosis, when first used with a subject, usually requires a longer time before results are obtained. Third, it is easier, requiring less effort on the part of the experimenter;

and it is easier for the beginner to learn. Fourth, it can be employed on a larger percentage of subjects with success at the start than can the usual methods of sleeping hypnosis. Fifth, if for any reason sleeping hypnosis is desired, one can easily change to the methods of producing the sleeping state with greater chance of success if the first suggestions by the method of waking hypnosis have been successful.

The technique of waking hypnosis which I employ may be described in part by contrasting it with the usual methods of sleeping hypnosis. To hypnotize by the most usual sleeping method one begins by explaining to the subject the psychological conditions of normal sleep. One calls attention to the part played by a lessening of external stimuli, especially light and sound, by the concentration of attention on some one simple situation, as in the classic method of putting oneself to sleep by counting sheep, and by the sleep-producing effect of slight monotonous stimuli. One may speak of the way in which the mother puts her child to sleep, and of the sleepiness that often comes upon a man while in the barber's chair, experiencing the manipulations of the barber. The hypnotizer explains that he is about to employ similar methods to induce in the subject first a drowsy condition and finally a condition of deep sleep, like normal sleep except that the subject will always be conscious of what the operator is saying. Then, in terms of the usual immobility of the body during sleep, contractures may be explained. In terms of normal dreams, illusions and hallucinations occurring in the hypnotic sleep are made clear. Amnesia that may follow the hypnotic sleep is compared to the amnesia for one's dreams that usually follows waking from natural sleep. Somnambulism in the hypnotic sleep is compared with the somnambulism that sometimes occurs in natural sleep. Then the hypnotizer proceeds to suggest drowsiness in various ways. He may have the subject gaze fixedly upon a bright object and at the same time suggest that a feeling of drowsiness will begin to appear. If the eyes soon close so that the subject cannot open them, the operator says this is because of the sleep that is overcoming the subject. Passes may be used, accompanied by suggestions of sleep; and so on, according to old and familiar methods. The subject manifests increasingly the external signs of drowsiness, and actually begins to feel drowsy and sleepy; and finally he may fall into the somnambulistic state, though more frequently stopping short of this.

Now, in all this there have been the following features: first, a preliminary explanation in terms of sleep; second, a continued

suggestion of sleep by direct and indirect means; third, an experiencing by the subject of the familiar symptoms of drowsiness and sleep; and fourth, some of the external bodily signs of drowsiness and sleep. Those who, like Münsterberg, Sidis, Coriat, and Prince, as referred to above, explain hypnosis primarily in terms of concentration of the attention and of dissociation, without reference to sleep, would not give a preliminary explanation to the subject such as I have described; but the other three points which I have mentioned would apply in most cases to their practice of hypnotism. In what I call waking hypnosis, however, all four of these features are absent: sleep is not mentioned in the preliminary explanation to the subject; sleep is not suggested, directly or indirectly; the subject experiences neither drowsiness nor sleepiness, if we may trust his introspective account; and there are present none of the objective indications of drowsiness or sleep.

The closest approximations to my personal technique that I have found in the literature of the subject are the methods of Wingfield [44] and of Coué as described by Baudouin.[45] Wingfield's type of preliminary explanation to the subject is directly in terms of dissociation without reference to sleep, and the same is true of Coué's preliminary explanation, which, however lacking on the scientific side, is good for practical purposes. Examples of involuntary ideo-motor action may be given, such as the tendency of the hand to illustrate a spiral if one is attempting verbal definition of it. Examples of absent-minded actions and of lapses of memory may be given, as illustrations of dissociation in the waking state. Then, without any reference whatever to sleep or to drowsiness one may proceed to an artificial manipulation of the subject's attention so as to produce by direct suggestion the various dissociative effects that one may desire. Coué's methods of producing contractures are the ones that I begin with, sometimes using Chevreul's pendulum for a preliminary exercise with difficult subjects. Contractures can easily be produced with practically all subjects by Coué's methods, and those who are familiar with them, and especially those who have been subjects, are agreed in calling the state of the subject a waking state.

Wherein then does my method differ from that of Coué and Baudouin, and that of Wingfield? In the practice of waking hypnosis I part company with Wingfield as soon as his preliminary explanation to the subject is finished, for he then begins to suggest

[44] *Introduction to the Study of Hypnotism*, 2d edition, London, 1920, pp. 49ff.
[45] Baudouin, *Suggestion and Autosuggestion*, Pt. III, Chs. 2, 3.

sleep, or at least drowsiness. His first typical suggestion to the subject, while the subject gazes at a cut-glass crystal, is, "Let your eyes get as heavy as ever they can, and let yourself become as drowsy as possible."[46] Coué remains within the field of waking hypnosis so far as the four points mentioned above are concerned. But I have been interested to extend the method and to produce by the same sort of direct suggestion the characteristic phenomena usually associated with deep sleeping hypnosis, such as amnesia and post-hypnotic phenomena, without the induction of sleep any more than in the case of the contractures of Coué's exercises. As has been pointed out above, Coué does not do this, and Baudouin regards sleep as essential in the production of amnesia.

Though Bernheim has recognized and asserted, as has been noted above, that *all* the hypnotic phenomena, including amnesia, can be produced in the waking state, I have not found in the literature any description by him, or by anyone else, of experiments in waking hypnosis involving amnesia and post-hypnotic automatic writing of the forgotten experience, or involving subconscious solution of problems given during the state of waking hypnosis, with amnesia for the problems produced immediately, and the answer given by automatic writing. Such experiments are exceedingly simple; and it is because of their simplicity and ease of performance, and at the same time because of their value for instructional purposes, that I venture to speak of them and to ask if they do not deserve to become a part of the standardized technique of instruction in the fundamentals of abnormal psychology, just as the mirror-drawing experiment, for example, has become universally included among the instruments of instruction in educational psychology.

Waking hypnosis may be used either in group or in individual experiments. The group experiment has two main purposes (aside from its therapeutic uses): first, to teach large numbers easily and quickly, through their own experiences as subjects or through observation of a considerable number of other subjects, the meaning of hypnosis; and second, to select the better subjects for individual experiments. After a preliminary explanation to a group of students regarding the chief principles of dissociation and of suggestion, direct suggestions to the group that their eyes, when closed, cannot be opened, or that their hands, if clasped together tightly, cannot be unclasped, will cause such contractures

[46] *Op. cit.*, p. 51.

in a considerable proportion, if not in the majority or even all, of the group, if made properly, as a little experience enables anyone to make them. I recently obtained results with 100 per cent of a group of 12, and a few months ago I obtained results with 24 of a group of 28. In no instance have I failed to get results from some members of the group.

In individual experiments the meaning of dissociation and the fact of the independent functioning of dissociated "neurograms" (to use Prince's term) may be illustrated by suggesting to the subject in the waking state amnesia, for example, for his name, and then by causing, through appropriate suggestions, automatic writing of the name while the amnesia still persists. By proper suggestions to a good subject one can cause automatic writing such that the subject is not aware either of what his hand is writing, or even that his hand is writing anything. To do an experiment like this by waking hypnosis takes only a short time, seems very matter of fact, and can be done with subjects who have never been hypnotized before. If one wishes in this connection to illustrate how the planchette works, a planchette may be substituted for pencil and paper. In elementary classes this is worthwhile. It was Gurney, as James relates,[47] who first conceived the idea of using the planchette to "tap" the subconscious processes involved in post-hypnotic suggestions; but Gurney used sleeping hypnosis for this purpose.

Such an experiment as I have described would illustrate what Prince devotes considerable space to, in *The Unconscious* (pp. 15ff), namely, the conservation of forgotten experiences. An experiment designed to show the independent functioning of dissociated neurograms in a greater degree is Prince's experiment in subconscious calculation (p. 96). This experiment, however, can be done by means of waking hypnosis, with subjects never before hypnotized. With a subject in whom amnesia can be produced quickly in the waking state, a problem in mental arithmetic may be given, for which amnesia is produced immediately, before there is time for any effort at solution. The suggestion may be made that the answer will be written automatically at the end of five minutes, with complete amnesia for the problem persisting during this time. In the working out of these suggestions we have an illustration of the simultaneous activity of the dissociated cerebral processes involved in solving the arithmetical problem, and of other cerebral processes involved in conscious attention to the class discussion or to some assigned task. Pro-

[47] *Op. cit.*, p. 615.

fessor Woodworth refers approvingly to one of Prince's experiments of this sort, with a subject, however, who has a double personality; and Professor Woodworth says, "It is weird business, however interpreted, and raises the question whether anything of the same sort . . . occurs in ordinary experience."[48] If the experiment is done with a subject never hypnotized before, selected from the class, and in a completely waking state, there is nothing in the least "weird" about it; and it answers the question which Woodworth asks in the last part of the sentence quoted above, being evidence that "separate [cerebral] fractions of the individual"[49] can and do function independently and intelligently at the same time, in strictly normal and healthy subjects.

The student often gets the impression from a study of Prince's book, though Prince does not intend it, that such experiments in hypnosis as are described, illustrating, for example, subconscious computation as in the case just referred to, can be performed only with very exceptional or even psychopathic subjects, since the subjects of most, if not all, of the experiments described in the book are sufferers from some of the forms of mental disease, as was the case with Charcot's subjects. Among the subjects described by Prince, M——l has psycholeptic attacks (p. 33), Mrs. W. has phobias (pp. 34f), Mrs. E. B. suffers from traumatic hysteria (pp. 35, 36), another subject has hysterical tics (p. 37), and the next two subjects mentioned, Miss B. and B. C. A. (p. 39), are cases of multiple personality. These are all of the subjects of the first group of experiments in hypnosis described in the book (pp. 31-39). To produce exactly the same results with healthy subjects selected from a class of students, and by means of hypnosis in a completely waking state, is much to be preferred.

Subconscious perceptions (discussed by Prince on pp. 52ff) may be illustrated in the manner that Prince suggests, but by means of waking as well as of sleeping hypnosis. "A simple method," Prince says, "is to ask a suitable subject to describe the dress of some person in the audience [or class], or of objects in the environment; if he is unable to do this, then to attempt to obtain as minute a description as possible by automatic writing or verbally after he has been hypnotized" (p. 53). I have frequently obtained through waking hypnosis correct and detailed descriptions of what had previously been entirely beyond recall regarding such subconsciously perceived objects.

An interesting experiment to illustrate the reality of pain

[48] *Op. cit.*, p. 560.
[49] *Ibid.*

in connection with functional diseases, is to cause pain in the subject's hand, or a toothache, or any sort of pain that one may desire, which is to continue for a minute, or any other specified length of time, after the suggestions of the experimenter cease. Amnesia may then be produced regarding the cause of the pain, and the subject, perhaps in embarrassment, will writhe with pain, knowing nothing as to the cause of it, until the suggested time has elapsed. Such an experiment, like the others which I have described, obviously can be performed effectively in waking hypnosis only with subjects in whom amnesia can be produced by direct suggestion in the waking state. And when amnesia can be produced readily in the waking state, post-hypnotic suggestions of all sorts can be illustrated as well as in deep sleeping hypnosis.

The step to effective autosuggestion, or autohypnosis, is shorter from waking than from sleeping hypnosis. My usual routine in giving a first lesson in autosuggestion is first to close the eyes of the subject, then to produce contractures of the hands, and then to produce analgesia in one hand or arm—all by direct suggestion in the waking state. Then I ask the subject to produce the same results by his own suggestion to himself. After he has done this, instruction may be given in the effective use of autosuggestion in various practical ways. I recently gave an interesting lesson in autosuggestion to one of my students. When he had produced analgesia in his right hand by autosuggestion alone, he was still unconvinced by the test of pinching with his left hand, and he asked for a needle. I gave him one, properly sterilized. He pricked his right hand repeatedly, so that the blood flowed from each needle wound, before he could fully satisfy himself that he had actually learned to produce analgesia by autosuggestion. As a final illustration of the practical applications of the principles of waking hypnosis, I removed by direct suggestion a headache of which the subject had complained at the beginning of the experiments. A further lesson is apparently needed by this subject, however, before he will be able to use autosuggestion effectively in practical ways; for he reported two days later that he had been able to produce contractures by autosuggestion, but not analgesia, when working alone.

The following description of a series of experiments in waking hypnosis carried out during a single class hour, on the first occasion of reference to hypnosis, shows what can be done in a short time. If more time is at one's disposal, variations and elaborations of such experiments are possible. At the end of one class hour I did a group experiment with the whole class, of fifteen students, in order to select the better subjects. Then, at the

beginning of the next class period one of these better subjects volunteered for individual experiments. The subject selected was a man of about thirty-five, never before hypnotized (except in the group experiment of the day before), and with a good history of physical and mental health. I first tested him to see if amnesia could be produced in the waking state. I readily produced amnesia for his name, with the suggestion that his hand would write it, while the amnesia still persisted. Pencil and paper were then provided, and his hand was concealed from his view, behind a screen. His hand immediately began to write his name. When the name was about half written the subject spoke up to say that he was sorry that the experiment did not seem to be working. After the name was completely written, and after amnesia for his name disappeared in five minutes as had been suggested, his writing was shown to him. The genuineness of his surprise and interest may be easily imagined. I next tested his normal ability in mental arithmetic, finding it fair. Then I made preliminary suggestions to him of the waking hypnotic type. I explained that I would give him a problem in multiplication, which he would solve subconsciously and the answer of which he would write automatically, with amnesia all the while both for what the problem was, and for the fact that a problem had been given to him. I then said, "Multiply 175 by 25," and I *immediately* thereafter caused amnesia for the figures and for the fact that a problem had been given. Then, testing his normal conscious attention to the class discussion, which I continued, by asking him miscellaneous questions, I allowed time for the subconscious computation of the problem and for the automatic writing of the answer. His hand wrote 4,325. The correct answer to the problem is 4,375. In tests given to the subject in the solution of similar problems before dissociation had been produced in waking hypnosis, similar errors had occasionally been made. I have in general not found subconscious computation either more or less accurate than the conscious solution of similar problems. I next caused by suggestion a sharp burning pain on the back of one hand, which I touched with a pencil. Amnesia for the cause of the pain was produced, and the pain remained constant for a minute, as had been suggested. I had produced analgesia in his right hand as one of the preliminary experiments. I have found it generally more difficult to cause pain by suggestion than to cause analgesia.

As a final experiment, towards the close of the hour, I illustrated subconscious perception. I tested the subject's memory

for details of the clothing of a man on the back seat of the classroom, a man, however, whom the subject had talked with earlier in the day. Finding him unable to recall any details whatever of the man's clothing, I tried the method of automatic writing without the use of hypnosis, and got an imperfect description. Then, through waking hypnosis I obtained a detailed and accurate description of the man's clothes. He persisted in saying that the man wore a white shirt with a dark stripe in it, in spite of suggestions from me that he would gradually come to recall it more accurately. To me the shirt seemed to be pure white; but after the termination of the experiment I discovered that there had originally been a dark stripe, which had faded out to such an extent that it was not visible to me at a distance of ten feet.

My notes contain descriptions of numerous experiments of a similar sort performed in connection with a course in abnormal psychology which I give each year. The experiments described above were done towards the end of a course in applied psychology which I gave at the University of Wyoming during the summer of 1923, the purpose of the experiments being to illustrate points in the topic of "Psychology and Medicine." In such a course little time could be allowed for such experiments, and consequently I was interested to see how much I could crowd into a single class hour.

In conclusion I might add that I am interested in the employment of the ordinary type of sleeping hypnosis for some purposes, especially in therapeutic work. I am interested in the sort of psychoanalysis by means of hypnosis which Dr. Hadfield, in England, has called hypnoanalysis;[50] and in attempting to remove phobias, for example, through hypnotic exploration of childhood, or later, amnesias, I have thus far preferred the sleeping type of hypnosis. However, I almost invariably begin the induction of sleeping hypnosis by the method of waking hypnosis described above; and I do not begin to suggest sleep until suggestions of contractures in the waking state have been effective. In this paper I have chosen to limit my discussion to waking hypnosis, and to emphasize its possibilities; for, if its possibilities were generally recognized, as is very obviously not the case, much of the present disinclination among psychologists to the use of hypnosis for experimental purposes would, I believe, entirely disappear.

[50] See H. C. Miller (editor), *Functional Nerve Disease*, London, 1920, Ch. 5, by Dr. Hadfield. Dr. W. S. Taylor has introduced the term "hypnoanalysis" in America through recent articles in this journal.

DISCUSSION

PORTEUS TESTS AND SOCIAL INADEQUACY

A RECENT article [1] in this journal describes an investigation involving the application of the Binet-Simon and Porteus Maze tests to four groups of cases who had passed through a psychopathic hospital. These four groups consisted of constitutional psychopathic inferiors, dementia precox cases, feebleminded and a "nonpsychotic" group. The last named group consisted of individuals to some degree maladjusted but "judged to be not insane nor mentally defective nor psychotic." The results are presented in the form of tables of group performance. The median Stanford-Binet test age of the nonpsychotic group was 14.08 years—a level which the authors remark to be "about what recent studies have shown to be the general average of intelligence." The group of constitutional psychopathic inferiors tested slightly below this level. The dementia precox cases tested lower still, and the feebleminded averaged 10.21 years. In the maze tests the constitutional inferiors tested a year below their Stanford age and the nonpsychotic almost two years below. The feebleminded on the other hand tested about a year above their Binet age.

Upon these results the authors have based some criticisms of a statement which they quote from one of my publications [2] to the effect that the maze tests are an aid to evaluating temperamental qualities and that they "do detect the mentally unstable." They seem to consider it inconsistent with these claims that the nonpsychotic maladjusted, being presumably nearer normal levels, should test lower than the psychopathic inferiors. Since the maze tests are supposedly tests of social adaptability and the cases passing through a psychopathic hospital are socially maladjusted, the inference that the writers appear to have drawn is that the constitutional psychopathic inferiors should have scored much lower in the maze than a year below their Binet level.

In the first place it is only fair to state that the claim made for the maze tests that they "do detect the mentally unstable" was made in reference, not to the mentally unstable in general, but to a particular class—the mentally unstable defective. The

[1] "A Comparison of the Stanford and Porteus Tests in Several Types of Social Inadequacy," by Ethel L. Cornell and Gladys L. Lowden. JOURNAL OF ABNORMAL PSYCHOLOGY AND SOCIAL PSYCHOLOGY, Vol. XVIII, No. 1, p. 33.

[2] "Porteus Tests—Vineland Revision," by S. D. Porteus. *Publications of the Training School*, Vineland, N. J., 1919, No. 16, p. 5.

specific application of the tests to this type of feebleminded is shown by the following passage taken from the publication from which Cornell and Lowden quote: "The question now arises whether we have tests which examine not mainly intellectual deficiency, but rather the capacities which are characteristically lacking in the mentally unstable, the inert, the impulsive and over-emotional types which Miner subsumes under the name 'conative feebleminded'." These conative feebleminded are individuals, not necessarily psychotic or psychopathic, who often score comparatively well in tests of the Binet type but who have such inferior personalities that they exhibit an instability of adjustment to life's ordinary situations. The maladjustment may be due to inertia or passivity; it may be related to indecision and irresolution of character and lack of normal initiative and ambition. There may be too great an openness to suggestion or a certain bluntness of sensitivity to failure and this inferior power of auto-criticism may lead to impulsive and heedless action. As a result of such temperamental defects we find these cases making much less progress at school than their general level of intelligence warrants. They show very little practical ability and thus fail to develop any useful dominant interests. They are so lacking in drive and initiative that they require constant stimulation and support. If either of the parents happens to be of a nervous temperament the home adjustments also are sure to be unsatisfactory. Ultimately the feebleminded institution becomes the final destination of many of these cases—not because it provides the proper environment but often because it is the only place which can accept the responsibility of caring for such cases. Once admitted to the institution they usually drift from one occupation to another. Their high mental age is readily overlooked since it has no practical significance and they finally find some comparatively low grade routine work where they labor side by side with and under the direction of feebleminded individuals much below their so-called "mental age". It is to such cases that the term "mentally unstable" or "ill-balanced feebleminded" has been applied. As a matter of fact "mentally unstable" is not a proper descriptive term, no better in fact, than "constitutional inferior." Since there is little evidence of abnormal functioning or disease of the mind the term "psychopathic" is also inapplicable. In short they constitute a type of inferior personalities exhibiting more or less general temperamental inadequacy which is plainly a limitation of development and no more psychopathic in nature than

ordinary feeblemindedness is. "Temperamentally inadequate" would be a far better descriptive term for these cases than "mentally unstable." It is with regard to these cases of instability of social adjustment, and not to all types of the mentally unstable that the maze test applies with special value. Incidentally we might point out that these cases frequently become delinquent but their delinquency is not the result of a strong instinct insufficiently controlled but is rather an expression of their all-around inadequacy. I should as soon expect the maze test to apply to all criminal types as I would to find it of universal application to all cases of mental instability.

The validity of the maze test has been established through its application by the author during the last ten years to upwards of 6,000 cases, including normal white children, many groups of feebleminded, the epileptic, the deaf and dumb and to children of other races including the Japanese, Chinese, Portuguese, Hawaiians, and Australian aboriginals. Its value as an indication of social capacity has been proven by a statistical comparison of its results with measures of experience independently given. The correlations show that the maze tests are tests predominantly of planning capacity, prudence, and mental alertness in attacking a concrete task. It tests these qualities, not at the level of adult development but rather at moron or near-moron levels. There is probably no single series of tests apart from the Binet, upon which so much investigational work has been expended for the purpose of procuring the data to make its interpretation in terms of social adequacy possible. Under the circumstances it is hard, therefore, to restrain a little natural impatience with attempts to criticize the tests on the grounds that they do not succeed in doing things they were never intended to do, and which in other publications I have expressly stated their inability to do. Surely it is from a too simplified psychological basis that the expectation would arise that such a generalized term as "constitutional psychopathic inferior" would include only impulsive, irresolute, easily confused types with poor planning capacity. So far from being surprised that some constitutional psychopathic inferiors made normal scores in the maze, I should have been astonished if such had not been the case as otherwise it would have so directly contradicted my own experience as may be gathered from the following statement which I made in another publication.[3]

[3] "Studies in Mental Deviations," by S. D. Porteus. *Publications of the Training School at Vineland.* Cf. pp. 85, 86.

"Because of the necessity of requiring a rather simple proof of the fundamental mental capacities involved, it happens that not only certain delinquents but certain types of psychopathic children tend to score too highly in the tests. The maze test suffers the same disability as most other performance tests, *viz.*, it demands the sustaining of attention for a comparatively short period. In addition there is the constant stimulus of the presence of the examiner—in other words the tests present the characteristics of a short, well supervised task of a novel nature. This is precisely the kind of work that psychopathic children of good native ability excel in; but they lack the stability of temperament to sustain their interest in the longer, less novel and more arduous tasks of everyday industry."

This quotation should make it clear how small was the expectation that the tests would reveal all forms of mental instability. It was because of this tendency of the egocentric individual of quite normal mental alertness to score too highly in the maze that I have been very careful to state several times that "failure in the test is more significant than success." In common with other investigators I have stressed the greater importance of methodical observation of the child at work over the merely qualitative expression of test age.

Though the article under discussion purports to be a comparison of Stanford-Binet and Porteus ages in certain types of social inadequacy, singularly little is said of the Stanford-Binet. If, as the authors apparently believe, the maze test does not have particular value as regards its application to the constitutional psychopathic as a group, it is evident, although they have neglected to point it out, that the Stanford-Binet has even less value. This is shown by the fact that the Binet age is above the Porteus age in 70 per cent of cases of the psychopathic maladjusted group and in 100 per cent of the nonpsychotic maladjusted group. The figures as presented by Cornell and Lowden merely prove that even the best psychometric tests have but a limited value in the examination of very many psychopathic cases. It is unfortunate for the psychometrist and particularly for the Binetist that this is so but the sad fact might as well be openly admitted. The proper realization of this simple truth might have prevented the writers from making such comment as the following: "Individuals of characteristic psychopathic makeup made perfect scores in Porteus, showing no evidence of instability there. . . . We have found cases of identical mental ages who

have high and low scores in Porteus respectively in spite of equally psychopathic behavior and equally serious conduct disturbances." We have more than a suspicion that identically the same thing could have been stated with regard to the Binet scores since, as the authors pointed out, these constitutional psychopathic inferiors test by the Binet only slightly below the average normal population. It can hardly be objected that the Binet is not used as a measure of social inadequacy since in many quarters a Binet I.Q. is used as an absolute diagnosis of feeblemindedness.

The authors also attempt to show that the Binet-Porteus average is not, as has been claimed, a better measure than either the Binet or Porteus age taken separately. This claim was made, not with regard to the psychopathic but to those who owe their social inefficiency to mental defect. As the group of defectives studied by Cornell and Lowden could not by any means be considered a representative group, the validity of the claim would not be seriously endangered even if the authors' observations were well founded. However, since their statement depends on no more substantial basis than the mere fact of previous court or institution experience as a measure of the degree of social inadequacy it need not be regarded seriously. Considering that not 5 per cent of all the feebleminded in the United States of America are or have ever been in institutions, it is surely assuming too much to consider that all the defectives with institution experience are more socially inadequate than all the defectives outside. A very slight acquaintance with homes for the feebleminded would have shown the writers that the inmates of institutions range all the way from almost normal social adequacy downwards. In other words, you may find individuals in institutions just as socially adequate as defectives outside—and defectives outside institutions just as socially inadequate as others inside. Previous court or institutional experience cannot, therefore, be taken as a criterion by which to measure the validity of the Binet-Porteus test average as a diagnostic measure.

But whilst Cornell and Lowden's article does not constitute serious criticism of the value of combining Binet and Porteus test ages, it might be well to summarize the results of a few investigations in which a more direct comparison of social or industrial competency with test age has been made.

A number of high grade adolescent defective males were given independent ratings as regards their social adaptability by the industrial and educational director at Vineland. These ratings

correlated .5 with Binet, .55 with Porteus, and .66 with the average of the Binet-Porteus test ages. For a similar group of girls the correlations were .73 Porteus and .59 Binet. A group of males from 15 to 30 years of age, rated by two independent judges by the Social Ratings Scale gave correlations of .60 Porteus, .60 Binet and .73 Binet-Porteus average. A similar group of females gave correlations of .75 Porteus, .69 Binet, and .79 Binet-Porteus average. A group of 100 unselected males, representatives of the higher grade population of The Training School when rated independently by the Social Rating Scale gave the following coefficients, Porteus .67, Goddard-Binet .62, Stanford-Binet .60, Goddard-Porteus average .77, Stanford-Porteus average .73. The validity of the Social Rating Scale as a measure of social inadequacy is shown by the fact that it correlated to the extent of .9 with estimates of social adaptability given without knowledge of the ratings by the Superintendent of the State Home for Girls at Vineland.

These results have been confirmed by the work of investigators elsewhere. Miss Ross of Edinburgh University, Scotland, found that the correlation of the Stanford-Binet with Handwork was .69 and Porteus .76. With a group of defectives in Baldovan Institution she found that the Stanford-Binet correlated with industrial efficiency .81, Porteus .81, and Binet-Porteus average .87. These results are confirmed also by Dr. Hurd in his investigations in the Manchester public schools, England, where the increased proficiency of the pupils in handwork was found to run nearly parallel to a rise in Porteus I.Q.'s.

Another point of criticism of the maze test referred to by some writers is its coefficient of reliability. Professor Mackie of Sydney University, quoting results obtained by Phillips, states that the maze tests are unreliable. Considering that the maze test is intended to measure, amongst other things the varying ability of individuals to improve with practice it is indeed strange that any investigator should consider an ordinary coefficient of reliability of any significance in tests of this nature. The practice of allowing repeated trials was deliberately introduced as a means of estimating improvement with practice. The procedure of allowing each subject, regardless of his age, to work right through the series from the lowest test is adopted for the same purpose. Because individuals do vary in their capacity to profit by experience it is wholly to be expected that these variations will appear when the test is applied the second time to the same group of

children. Yet a test is supposed to be very reliable if the second application of the test gives results identical or nearly identical with the first application. It is obvious when children test high by the test on the first application and low on the second application the test is probably unreliable. This is not the case in the maze test. It is rarely that a child has a lower score on the second application. In other words the variations in the score represent an actual improvement with practice. When the practice effects are allowed for, the Porteus maze test is one of the most reliable series in present-day use. Morgenthau, in her study entitled *Some Well-Known Mental Tests Evaluated and Compared,*[4] found that the coefficient of reliability for the maze test was .95. It is surely obvious that by allowing only a single trial to each test or by beginning the examination with the test at the chronological age level of the child the so-called reliability of the test would be greatly increased. It is obvious that if only one trial be allowed, say for the twelve year test, or a rigid time limit be employed, then the child who failed on the first application of this test would probably fail on the second, giving a high measure of reliability. When as is the case four trials are allowed this means that in two applications of the test the subject has eight trials in all. By this plan the varying ability of subjects to readapt their methods and to improve with practice can be taken account of. Again, to score the test on a time instead of accuracy basis would alter its character and value altogether. Some investigators who have followed me in the use of the printed maze as test material do not appear to realize the fact that when time and not accuracy is the basis of scoring the test it is no longer a test of prudence but of a particular form of mental alertness. It is the combination of planning capacity with prudence in carrying out the plan that constitutes intelligent adaptation. Personally I should be a very good chess player if it were not for my opponent. My plans are excellently laid and show fine foresight but I do not take sufficiently into account the fact that my opponent also has a plan which rarely fits into mine. Bold plans with an imprudent inattention to details in their execution are rarely successful. In real life just as in the maze situation the temperamental factor is fully as important as the intellectual. It would seem wholly undesirable, therefore, to ruin the validity of the tests as measures of social adaptability to insure a somewhat insignificant reliability.

[4] *Archives of Psychology*, No. 52.

I hope it will not be considered that I am holding a special brief for the defense of the maze test or in fact for mental tests in general. Personally I have never been imbued with a simple child-like faith in the extreme validity of any mental tests. I have repeatedly stated my belief that it is the whole personality that should be studied before we can make reliable diagnoses and that the test age may be absolutely insignificant in comparison with the importance of other factors. All that the maze test does is to measure approximately the ability of the individual to use planning capacity in a task at or about moron levels and to readapt his methods in the face of increasing difficulties *as far as his temperament will allow him to do so.* It was because I realized the importance of this relation of temperamental deficiency to successful adaptation that I devised these tests. I do not even consider the maze tests an adequate measure of temperament. They do, however, give us a fairly reliable estimate of the person's suggestibility, impulsiveness, irresolution and tendency to become nervous and confused in a task new to experience.

I admit also that in certain individual cases, because of the variety of causes for failure, the test is occasionally difficult of interpretation. I have publicly stated the fact that after ten years of intensive work on the maze tests I can tell, in a very short time, practically all that is definitely known and worth knowing about the tests. This admission does not arise from any excess of modesty on my part as I consider that the majority of devisers of other tests could perform a similar task with regard to their own tests in considerably less time.

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REVIEWS

ABNORMAL BEHAVIOR: PITFALLS OF OUR MINDS: AN INTRODUCTION TO THE STUDY OF ABNORMAL AND ANTI-SOCIAL BEHAVIOR. By Irving J. Sands, M.D., Instructor in Neurology, Columbia Univ. Med. School, and Phyllis Blanchard, Ph.D., Psychologist, Monmouth County Mental Hygiene Clinic. New York. Moffat, Yard & Co., 1923. Pp. ix + 482. Price $4.00.

This is of the nature of a textbook of neuropsychiatry as understood of late so broadly, but adapted particularly to novices therein and to "social workers." The space is taken up largely with 137 brief case histories of conduct-disorder and discussion of these, often with much insight. The subtitle, "Pitfalls of Our Minds," seems to have no special appropriateness.

The contents proper (besides a brief introduction and a good index) are divided into thirteen chapters, named thus: The instructive and emotional basis of behavior; Emotional conflicts in the causation of conduct disorders; Intellectual capacities and their relation to behavior; The physical basis for behavior and the influence of somatic diseases upon conduct; The relation of personality-types to behavior; The rôle of the psychoses in the causation of anti-social conduct; Borderline mental disorders as causes of abnormal conduct; Epileptic manifestations in behavior difficulties; The problem of drug addiction; The unbiological behavior of the suicide; Educational maladjustments; Vocational and industrial maladjustments; and Modern methods for the prevention and correction of conduct disorders.

Chapters one, three, and four together constitute an excellent popular description of the physical and mental bases of behavior, going rather well into the essentials of the elementary physiology and psychology, normal and defective, concerned. Chapter two warms up the related Freudism with a number of case histories (after the manner of Healy) as appetizers. In fact the whole volume reeks, but not nauseatingly, of libido and all that sort of thing after the obsession, now fortunately less fashionable, of several years ago. Not that the authors fall into that persuasive pitfall of their minds that gives all energy a sexual reference, immediate or remote, but that sexuality is given too much discredit in misbehavior.

Personality make-up is well discussed in chapter five, and the various trait-paradigms (those of F. H. and G. W. Allport, of F. L. Wells, of Hoch-Amsden) set forth usefully for comparison. Some popular novels are ventilated a bit. The reader is reminded that "morals are a matter of geography," as obviously they vary greatly with the calendar. Each of the chapters dealing with more or less separate phases of psychiatry is a really excellent discussion. It would be hard to name a treatise on these subjects for unprofessional and semiprofessional consumption in which the treatment is more clearly expressed, more scientific, or more—contemporaneous.

No chapter will meet with wider approval from professionals than the last, devoted to prevention and correction. In it are discussed many topics

timely for "applying" psychologists and sociologists. Among them, after some historical notes, are the mental clinic; the idea of reforming criminals instead of getting revenge on them; the influence of psychiatric knowledge on the treatment of delinquency, and on its prevention; mental clinics for children and for delinquents; the Glück plan; the "Commonwealth plan"; baby clinics; and mental hygiene education in general and in its particular varieties.

Not the least useful part of the book, perhaps, are the brief lists of supplementary readings. This little bibliography ignores, however, mostly all articles and books save those in English. There is a good index, beside analytical contents.

The book is another example of the waste of space in printing sociologic case histories that has been so obvious of late in many books of import similar to this book. No two human behaviors are alike outwardly and in motive; to publish them seems to savor more of catering to a love of a sort of gossip than to scientific usefulness. But the book certainly as a whole is a valuable one, even at a high price.

GEORGE VAN NESS DEARBORN.
United States Public Health Service Reserve, New York.

THE MEASUREMENT OF EMOTION. By W. Whately Smith. With foreword by William Brown, M.D. New York. Harcourt, Brace & Co., 1922. Pp. 184.

EXPERIMENTELLE UNTERSUCHUNGEN ÜBER DAS GEFÜHLSLEBEN DES KINDES IM VERGLEICH MIT DEM DES ERWACHSENEN. Helga Eng. Beiheft 30, Zsch. f. angewandte Psychologie, 1922. Pp. 258.

William Brown in his foreword to *The Measurement of Emotion* asserts that the most characteristic features of modern psychology are (1) the special attention given to the facts of emotional consciousness, and (2) the persistent endeavor to obtain a quantitative statement of results. The present work by Whately Smith may be taken to illustrate these two features. The writer employs the word-reaction method, but confines himself to a study of reaction times and disturbance in reproduction in relation to the psychogalvanic reflex. The procedure parallels in most respects that of Jung. Instead of finding however, that the affective tone connected with "strong" words is always prejudicial to memory, he finds that affective tone, as detected by the psychogalvanic reflex, exerts an influence in two diametrically opposite directions. The fact that a given word evokes a well-marked affective tone may lead to its being remembered better than a less intensely toned word, or may lead to its being forgotten more quickly. The investigator finds, likewise, that nonsense syllables, presented visually, evoked in varying degrees the psychogalvanic reflex, even though no associations are asked for. The reproduction of these syllables is influenced by the affective tone in two different ways; the syllables which are attended by a large deflection are either more readily remembered or more readily forgotten than those attended by a slight

deflection. The writer concludes that "nonsense" syllables are not devoid of meaning; unconsciously they are associated with pleasantly or unpleasantly toned ideas.

The chief argument of the book is that there are two types of affective tone—the positive and the negative. The positive tone is of the facilitating, harmonious, and of the consciously pleasant variety; negative tone is related to inhibition, with drawing, repression, and unpleasant states of consciousness. The "innerness" of the response word is associated with negative affective tone. The effect of alcohol is in general to raise the threshold of emotional response, but in cases where an emotional response is present at all the reaction tends to be extreme: there tends to be a large response or none at all.

Very little is contributed in this book to the theory of emotion, beyond the contention of a two-dimensional affective tone, and nothing that is new to the technique of investigation. The writer throughout oversimplifies the complexities of emotional consciousness. Thus, for example, he does not deal with the numerous qualitative differences which exist between affective states; he subsumes them all under "positive tone" and "negative tone". Notwithstanding the hypothetical nature of his work, and of all work on the measurement of emotion at the present time, he gives to his data an over-refined quantitative treatment. It would be of more value at this stage to have a critical formulation of the fundamental principles of emotional measurement than to have any amount of such misguided precision.

The investigation by Eng is more thorough and more critical. Its purpose is (1) to discover what circulatory and respiratory changes accompany affective states, and (2) how these changes differ in children and in adults. Records of blood volume in the arm, of pulse, and of respiration were taken under conditions of simple attention, mental work, stimulation through pleasant and unpleasant odors and tastes, intellectual activity, and aesthetic contemplation. One hundred pages of tables and graphs give in detail the results of these experiments. In general there are six affective states which may be distinguished from these records: (a) attentiveness—characterized through decreased volume, retarded and weaker pulse, slower breathing; (b) activity *(Tätigkeitgefühl)*—increased volume, stronger and accelerated pulse, rapid breathing; (c) unpleasantness—decreased volume, weaker and usually retarded pulse, deeper and irregular breathing; (d) pleasantness—increased volume, stronger and usually accelerated pulse, shallower and more rapid breathing, (e) depression—decreased volume, weaker and slower pulse, retarded breathing; (f) excitement *(Erregung)*—increased volume, stronger and more rapid, but often irregular pulse, accelerated and shallow breathing. Eng's findings concerning the symptomatic changes for each of these states are given with a comparison of the findings of previous investigators. The results suggest to her a tri-dimensional theory of affection not unlike that of Wundt.

A valuable contribution is that relating to "spontaneous" curves, *i.e.*,

those curves which register while the subject is in a passive state. From these records can be read the effect upon the subject, for example, of watching the experimenter prepare a disagreeable stimulus, or of the emotional components in ordinary thought processes. Such records are checked subsequently with the report of the subject concerning his mental state. It is in this direction that children and adults are found most radically to differ. "Spontaneous" curves indicating unpleasantness are more infrequent in the records of children than of adults. When they occur in children they are usually connected with actual sense impressions, and are of short duration; whereas adults frequently show such curves in connection with thought processes of a more personal type, and such curves are frequently of long duration. "Spontaneous" curves indicating pleasure are often met with in the records of children; these likewise are aroused chiefly through sense impressions. When such curves appear for adults they are produced only by subjective trends of thought. In all experiments in which definite stimuli are used the curves for children do not differ from those of adults.

GORDON ALLPORT.

THE EUGENIC PROSPECT: NATIONAL AND RACIAL. By C. W. Saleeby, M.D., F.R.S. Edin. F.Z.S. New York. Dodd, Mead & Co., 1921. Pp. 239.

This volume, "dedicated to the cause of Anglo-American Friendship for the Weal of All Mankind," is a continuation in a way of "The Whole Armour of Man," published in 1919. "Its pages are dominated from first to last by the series of revelations and inspirations and instructions which America, our tremendous offspring, offers to the visitor and student from these Mother-islands." "Only such a mother could have such a daughter."

For twenty years a progressive and royal follower of the pioneer ideas of Francis Galton, Dr. Saleeby has long been engaged in setting forth many of the nurtural aspects, "environmental," of the eugenical problem as more or less distinct from and yet complementary to those aspects that are inherently hereditary, "natural." This book consists of essays, thirty-four in number and divided into five groups, respectively named "The Future of the Race," "Let There Be Light," "The White Plague," "National Dietetics," and "The Next Necessity."

There is little in the book that does not underlie social psychology, although there is much that underlies it more indirectly than most of the contents of the JOURNAL: *i.e.*, the volume is chiefly devoted to the hygiene of the body in other than its neuropsychiatric aspects. In the chapter "The Future of the Body," however, the increasing dominance of certain parts of the brain is pointed out, and the discussion ends with a page characteristic of this popularizer of eugenical science:

"Always the upward trend of evolution is towards that in which there is most of mind. In 1907, in a course of lectures in the Royal Institution on 'Biology and Progress,' I ventured to define progress as 'the emergence and increasing dominance of mind.' . . . We live in a material world,

to which we must adapt ourselves or, indeed, and that is the deed of deeds, which we must adapt to ourselves—not merely moulding ourselves upon this 'sorry scheme of things entire' but actually able to 'grasp' and 'rebuild it nearer to the heart's desire.' Therefore, we abandon bodily mechanisms, as of tooth and claw, only to create superior mechanisms, to fly the Atlantic as never bird did yet, to sweep the deep as never shark or whale, to pierce the sky as never eagle's eye. We have abandoned the finite for the infinite. A bodily organ has its bodily powers, according to the strength of lever and pulley and what not. But the crescent mind and brain of man can harness Niagara or the lightning or the storm; there is no limit to his powers. By taking thought he cannot add a cubit to his physical stature, but he is the 'tool-making' animal, as Lichtenberger called him, and thus, in a 'sky-scraper' or aeroplane, he can add countless cubits to his stature. Bergson has written profoundly on this limitless substitution, by the mind, of extra-organic for our organic organs—if the clumsy phrase be allowed. Claws are dwindled to nails—but what of buzz-saws, and the acetylene drill? The modern eye tends to be myopic, but what of the spectroscope by which the physically short-sighted astronomer can see kinds of atoms in Sirius or Aldebaran? 'In the universe there is nothing great but man, and in man there is nothing great but mind.' "

The volume for several good reasons merits the attention of all students of the conditions underlying social and personal psychology, normal and morbid. Dr. Saleeby is "doing good work" spreading the gospels of well-living and of international harmony—two high ideals.

GEORGE VAN NESS DEARBORN.

United States Public Health Service Reserve, New York.

IS AMERICA SAFE FOR DEMOCRACY? By William McDougall, Professor of Psychology, Harvard University. New York. Scribners, 1921. Pp. viii + 218.

Professor McDougall's Lowell Lectures, "Anthropology and History or the Influence of the Anthropologic Constitution on the Destinies of Nations," are here brought out in one volume under the present title. The author has attempted to bring together in popular form the material dealing with the racial basis of politics and history. He has relied, as he must, upon much material that is impressionistic, a mere collection of subjective observations upon living and deceased nationalities in Europe, and also upon some data of more scientific value drawn from demography and from the psychology of individual differences.

In the opening chapters the writer gives a review of the theories of the anthropological influences in history. He presents the two standpoints, one which attributes all progress, practically, to the door of invention, opportunity, and culture; the other which puts its emphasis upon the biological and racial factors. Here he lays down his own thesis "that the great condition of the decline of any civilization is the inadequacy of the qualities of the people who are bearers of it." He points out the incom-

pleteness of the theories supported by Mill and his followers, who held for an almost unlimited possibility of human advancement and for the efficacy of education in making men capable of carrying forward and enhancing a high culture.

His first criticism of this theory he finds when he reviews the recent studies made by mental tests of the intelligence of our older as compared to our newer immigrant groups in this country. He is, from this evidence, convinced of marked differences in mental ability between the "races" of Northern and of Southern Europe, and, moreover, on the basis of such statistical studies of mental heredity as Galton and his followers have produced, he maintains that these differences are in large measure innate.

To elaborate his thesis of decided differences in European peoples, the writer passes on to other sources of information purporting to deal with the effects of climate, with temperamental differences, and with divergent development of various instincts. For instance, he informs us that the Nordics possess a greater dosage of curiosity than do the Mediterraneans, and thereby he accounts for the development of modern science by the Northern nations of Europe. To make this thesis sound, of course, he is forced to hold that the Greeks, who initiated much of the basis of science, were compounded from the Nordic and Mediterranean "races." Furthermore, when one comes down to individual cases, this race-thesis becomes pretty shaky. Were Galileo, Copernicus, Bruno, and others, then, Nordics who happened by the configuration of their ancestry to live in Latin or Alpine countries? Further, although the Mediterraneans and Alpines are weaker on the side of curiosity, they are much more given to the "herd instincts" than the Nordics, which makes them much more susceptible to popular passions, and hence does not make for true democracy, which, the author would have, is also the product of Nordic quality.

Moreover, the Latins are "constitutionally extrovert" and the Nordics "constitutionally introvert." And this in spite of the fact that England has been the seat of rigid empiricism throughout the modern period! Again, in the field of religion, the Southern peoples are noted for their objective worship, their interest in the color and sensuousness of the ritual of the church, whereas the Northern European takes more to the closet for reading and for contemplation in private of his religion.

Out of all this the author enunciates a *"law of the adaptation of the culture-species"* (italics are McDougall's), which is a restatement of his general thesis that the cultural contribution of a people is after all determined by its distinctive racial characteristics. Customs, social organization, the form of the family, the system of land tenure, the political and educational institutions, are all fundamentally set by the native traits of the people in question. Nurture, true enough, plays a part, but relatively only a minor one, never a determining one.

With his interpretation of intelligence differences and with his belief in marked divergences in certain instincts and emotions in the European "races," Professor McDougall goes on to point out the great need of

trained leadership to-day in view of our enormous industrial and political expansion, accompanied withal by the serious depletion, through sterility, of just those stocks in our population which are requisite for this leadership. He states his general standpoint on the matter as follows: *"Our civilization, by reason of its increasing complexity, is making constantly increasing demands upon the qualities of its bearers; the qualities of those bearers are diminishing or deteriorating, rather than improving."* (Italics the author's.)

In an appendix there is presented certain other concrete material to combat the notion of innate equality of races. Here, too, the writer severely censures the opponents of eugenics and expresses considerable concern over the decimation of the ranks of the better stocks in this country. He regrets the refusal of persons of these classes to do their social duty by producing large families to offset the increasing numbers of inadequately equipped persons of poorer stocks.

Besides the general criticism of the paucity of scientific material on alleged racial differences in European peoples, and in addition to the experimental and systematic criticisms that have been made of mental testing, one must reëmphasize the fact that the author has not rid himself of two major prepossessions, either of which would vitiate his conclusions in large part. The first is his constant confusion of individual differences within social classes and racial groups with differences between races as such. The second is the continued assumption throughout of an antiquated and outworn anthropology which posits the existence of three distinct races in Europe—the Nordic, the Alpine, and the Mediterranean. The work of Retzius and others on the side of physical anthropology, on the one hand, and the contribution of the school of cultural determinism on the other, have long since knocked this theory into a cocked hat. In this connection, moreover, his drawing upon the army test results is peculiarly unfortunate, for he hereby confuses the facts about nationality groups in this country with the intellectual status of the entire population from which they sprang in Europe. Furthermore, he makes these nationalities synonymous with this mythical classification of races made popular by the writings of Gobineau and H. S. Chamberlain.

In spite of these severe strictures upon this book, we must not forget our debt to its author for raising his voice in behalf of eugenics. Most Americans seem to prefer to continue in their myopia to the appalling increase in the numbers of feebleminded in this country. They lull themselves into easy comfort in the typical American naïve belief in the universal hocus pocus of education and social reform to solve all such difficulties. So far as the problem of immigration is concerned, there does appear to be considerable evidence that those samples of Southern and Southeastern European populations which have been flooding this country in the past twenty-five years are not qualified in culture or in intelligence, as measured by the tests, to participate in our American political and social life as well as were our older immigrants from Northern and Northwestern Europe. Whether these differences are eternal and innate is a

much harder problem to answer. Certainly, however, the results from the army mental testing and from extensive surveys of school children of these Latin and Slavic stocks ought to cause us some public concern and attention. If it is a matter of health, of educational opportunity, of stimulation of interest, and of the absorption of techniques and culture generally, it demands much more effort than we have up to the present given it. If it is a matter of innate intelligence (whatever that is) and differences in emotional-instinctive make-up, it behooves us to spend some time discovering more facts about the subtle nature and operation of these factors in social life than Professor McDougall has been able to give us. Therefore, whether one agrees in whole, or in part only, with the author's standpoint, it must be admitted that his discussion has given us matter for reflection of the highest social importance.

KIMBALL YOUNG.

DIAGNOSIS AND TREATMENT OF YOUNG SCHOOL FAILURES. By Helen Thompson Woolley and Elizabeth Ferris. Department of the Interior. Bureau of Education. Washington. Bulletin, 1923, No. 1. Pp. vi + 115. Price 10 cents.

The main aim of the Helen S. Trounstine Foundation, under whose auspices this Bulletin was prepared, is the "furnishing of additional aid which will make it possible to gather up and interpret to the public the work of educational and social agencies which have no resources to do it themselves." The experiment reported in this Bulletin, however, was the direct outgrowth of the work of the psychological laboratory of the vocational bureau of the public schools of Cincinnati—a laboratory which was established through the combined efforts of the Council of Social Agencies of Cincinnati and the public schools. At the beginning of the career of this laboratory in 1916, the task of diagnosing defective children and transferring them to classes for defectives was assigned to it, the aim being to discover these children when they were as young as possible, so that some real training could be given them. A later study of these retarded children was made which showed that 80 per cent of the children were twelve years old or older when they were first assigned to special classes, also that the average length of stay in the special class was a year and a half, too short a time for adequate training.

In the early part of 1917 many of the first and second grade failures were examined. Among them were found some whose school failure was great, but whose mental tests were above the limits usually set for mental defect. This group then was the group of "avoidable failures" or the backward children who could be helped by suitable training, and they had been discovered while still young enough to make the training valuable. Obviously, too, since the children were all alike in being school failures with, however, the mental ability to do school work, the cause for the trouble must lie in some outside factor—disposition, home conditions or physical conditions.

To study the conditions affecting these children, the laboratory recommended the establishment of an "opportunity class" of a group of sixteen of these children, the purpose of which class should be to complete the diagnosis and come to a conclusion as to the cause of failure, then, if possible, retrieve the failure, and return the child to the proper grade. Miss Ferris, being well fitted for the task, was selected to conduct this experimental class.

The study then continues with a detailed account of the first observation class, and a history of each child down to 1921, together with general conclusions based on the four years of study. It was sincerely felt that "small as the number of cases is, we believe that within this little group are found all the types of problem children which the primary teacher is apt to encounter. If, by telling as completely and vividly as we can the early school histories of these poor little neglected, thwarted, anxious, injured children, with their limited endowment, their physical defects, their home difficulties, and their warped personalities, we can help some teacher or social worker to understand the little failures she is trying to help, the result will amply justify our efforts."

As part of the preliminary work of the class, the home of each child was visited by Miss Ferris, the purposes of the class explained to the parents, and as much as possible of the personal history and the social and educational background of the children was learned. The room utilized for the class was inadequately supplied with necessities, but was made as attractive as possible and was separate from the school itself. For the mental examination, Binet tests were used in almost all cases, supplemented by the Pintner-Patterson performance scale and other mental tests and educational scales. Further data of a social nature were also gained through records of the laboratory and from other social agencies of the city.

An analysis of this group of children showed a variation in chronological age of from six years seven months to ten years and four months, but not one of them had accomplished as much academic work as is prescribed for the first grade, though six children were actually in the second grade and one in the third. The intelligence quotients of the first test varied from 75 to 95 for the group.

The general methods of teaching such children as individuals were given in some detail by the authors, followed by the detailed case reports of each individual child. These form the bulk of the article and are most interesting and valuable.

In the discussion of results given at the end of the article, the first point made is that many of the children of this class would never have learned to read at all if they had not had this individual instruction; in many of these children also was inspired a real love of learning and a desire for independent work, which one ordinarily considers as present only in children of higher ability. These two results are obviously worth while.

Another important result, perhaps allied to the above, is that while some of these children hated school when they first came to the observation class, and acted accordingly, after some months spent in this class they

loved it. Also these children learned to apply themselves to their work, pay attention well and take interest in what they were doing—proof of the value of the work.

From the point of view of the school and teacher three services were rendered; it took the most difficult of the problem children from the regular class; it gave these children the benefit of the doubt as long as there was any chance of success; and lastly, it gave an opportunity for a gradual diagnosis of the real nature of the difficulty.

In regard to the results of the mental testing, one very interesting fact was brought out by the study. In every instance, except those of the children who were ultimately shown to be feebleminded, and one of those who was suffering from special disabilities, the intelligence quotients rose while the children were in the observation class and fell after they left it. On the other hand, for the children who were ultimately classed as defective, in no instance did increase take place, and in three of the four cases there was a decrease even during the stay in the observation class. Various tentative reasons as to the cause of this situation were suggested, but the final decision was—"The only way in which we are able to interpret it is that the stimulating atmosphere which surrounds the children in that class actually increases their power of performance while it lasts. It is as though the constant, personal, stimulating influence of the teacher infuses into the children an alertness and a capacity for comprehension which they cannot command when thrown on their own resources. Whether the gain in power could be consolidated and made a permanent possession if the children could be left for a longer period in so favorable an environment, we do not know, though we are inclined to think it possible." If this is the true explanation of these facts, then it surely is a strong argument for better teachers and smaller and better equipped classes.

Five points to be considered in diagnosing young school failures are discussed briefly, namely: (1) the mental level of the child; (2) academic record of the child—regular or irregular attendance at school; (3) the child's state of health—this seems to be an important *contributing* cause to failure; (4) the general mental tone and attitude of the child, and under this heading are included mental distraction due to anxiety caused by poverty or unhappy relationships in the family, personality conflicts between the child and his parents or the child and his teachers, obsessions or fears—religious ideas or sex, special disabilities, character defects, and psychopathic conditions, the latter usually hereditary in such young children; (5) the heredity of the child.

In closing, the functions of the school of the future are discussed, and various suggestions made in regard to how these can best be fulfilled. "If the school of the future is to be equipped to meet problems such as those of the observation class, it is evident that a great extension of the extra-classroom facilities of the school must take place. Hundreds of children are failing in our schools for causes which lie primarily outside the classroom. These failures cannot be properly diagnosed, much less wisely treated, unless the school has facilities for securing mental and physical

examinations, social investigations, and family histories of the children. . . . The existence of psychological clinics, of school medical clinics, and of visiting teachers demonstrates the point." The school as the center through which medical and social problems may be referred to the agencies of the community who are fitted to care for them, is another point made as a constructive suggestion. In this regard the fact that the school has community-wide contacts and has a hold on the families through the children, gives it an advantage in dealing with the problems, their discovery and diagnosis, that no other agency can have.

In brief, then, the need is "to provide adequate clinics, mental, physical, and social; adequate school systems, and adequate institutions for the feebleminded and the psychopathic, who prove during their period of education unable to adapt themselves to conditions outside of institutions," and, it is concluded, "most of our reformatories and penal institutions could be done away with."

This study, a scientific study based on accurate data, carefully worked out, is an example of the type of study that must be made to prove to school superintendents and their committees, to judges and court workers in general, to social workers and to teachers, that, after all, *prevention* of trouble is cheaper and saner than *cure* of trouble and crime, and to impress legislatures with the need for adequate appropriations for *preventive constructive* work. It is to be hoped that other similar studies will appear in such quantities that even the most conservative man or woman will have to yield to the facts.

C. Elizabeth Dealey.

The Measurement of Mental Traits in Normal and Epileptic School Children. By J. E. Wallace Wallin, Professor of Clinical Psychology, Miami University, Oxford, Ohio. Miami University Bulletin, Series XXI, No. 8, April 1923. Pp. 175. Price 75 cents.

This Bulletin records the results of an experiment to test the relative abilities of normal and epileptic children. The experiment was carried out during five successive months. The normal children, so-called, were enrolled in the public school of Hopewell, N. J., in grades from the second to the eleventh. The principal and teachers of each grade selected two boys and two girls from each grade, each child ranked as "dull", "average" and "bright". The records of 76 pupils were finally included in the results, the others being unavailable for one reason or another, and of these children all were white except one. The two sexes were about evenly balanced in number. The average chronological age of the boys was 11.47 and the average school grade was 6.2, while for the girls the average chronological age was 12.3 and the average school grade was 5.9.

Thirty of the ablest epileptic children in the school department of the N. J. State Village for Epileptics at Skillman were selected for the other half of the experiment. For this group the average chronological age of the boys was 14.56 and the average school grade was 2.1, while the

chronological age average for the girls was 15.76 and the school grade was 2.3. None of the epileptic children did school work beyond the third grade, while 81.5 per cent of the normal children were in Grades IV to XI. Relatively it was found that the epileptics were decidedly more deficient pedagogically than mentally.

"The aim of the experiment was to measure under controlled conditions the comparative amount of change or development which might occur in various mental traits among various grades (bright, average, dull) of normal pupils of different ages and school grades and among epileptic school children during the same intervals of time and under the same experimental conditions." For this purpose a series of tests of uniform difficulty, in so far as possible, were prepared and given to the children at intervals of twenty-eight days between January 17th and May 9th, the time required to give all the tests to one group of children being approximately one hour.

The bulk of the Bulletin is made up of a description of the test materials, a statement of the purpose of each test, directions for administering each test, a presentation of the tabular data, and conclusions based on the more important results of each test, given at the end of each chapter. The following tests were given: (1) memorizing visually presented numbers—a test which aimed to measure the visual memory span for nonsense material (three-place digits); (2) rapidity of thinking—a spontaneous association test; (3) addition test, a controlled association test—the ability to add a series of numbers accurately and quickly; (4) controlled association-antonym test with supplied keywords, "considered one of the best tests of intelligence of the so-called association or selective thinking type"; (5) attention and perception test—speed and accuracy of perceptual discrimination in the crossing out of A's—to measure the speed and accuracy of perception and motor reaction and of the subject's ability to sustain attention; (6) observation—range of visual apprehension, a test which measures the rapidity or range of visual observation or apprehension during a limited exposure; (7) memory of logical and illogical associations—reproduction of sequents or antecedents in paired associates, a measure for finding the ability of the subject to retain twenty pairs of supplied associates; (8) visual imagination—the ink-blot test; (9) word construction—building words from six supplied letters, a test which requires for success a good-sized vocabulary, ability to spell and write correctly, fertility of invention or resourcefulness in combining the letters in a large number of ways, and ability to concentrate on the problem; (10) sentence construction—building sentences from three supplied words, a test of "literary imagination"; (11) recognition of illustrated postal cards, to test the degree of active attention, keenness of observation and power of retention; and (12) speed of motor reaction—writing circles.

The results of all these tests were very carefully tabulated and studied and certain general comparisons and conclusions drawn from them. In

the first place the tests showed the great inferiority of the "comparative efficiency" of the epileptics. For instance, in comparing the averages of the entire epileptic group with the averages of the entire normal group, it was found that in only one test (writing circles) did the epileptics reach 80 per cent of normal efficiency, in only four tests did they average between 51 and 67 per cent of normal efficiency, while in the remaining eight tests they did less than 38 per cent as well as the normals.

Thus Dr. Wallin concludes that "the epileptics, all of whom were backward or feebleminded, were inferior to the normals not only in the more intellectual and complex tests, but also (although less decidedly) in the simpler and more sensory tests." This conclusion is at variance with the conclusions reached by some other men, but is in agreement with the findings of still others. Various experiments for both sides are cited in this connection.

In referring to the bearing of the results of this experiment on the training of epileptics, Dr. Wallin writes:

"Our epileptics as a group—which may have been inferior to the non-institutional cases of epilepsy—certainly did make some improvement in most of the tests, although nearly all were suffering from convulsions at the time; and although many of them were feebleminded, relieved of the seizures, it is probable that the comparative records made by the epileptics would have been greater. The first treatment of epilepsy, whether hygienic or medical, should aim to prevent or abort convulsions, but care must be taken that a form of treatment is not used which will produce worse secondary effects than the convulsions themselves. So far as the educational treatment is concerned the inference from our mental tests, as well as from our school experiences with epileptics, is that the most profitable form of training to give the typical epileptic is sensory, motor and industrial in character, while the best form of occupation is outdoor employment. We question whether it is in the interest of the epileptic or of public economy to provide any considerable amount of literary training for the feebleminded epileptic and the epileptic with destructive seizures. The literary training may well be largely restricted to mentally well-endowed epileptics whose seizures have been aborted or at least do not produce mental impairment."

Various other conclusions in regard to sex differences and individual differences are cited, and, finally, the author feels that the tests have proven their worth as a means of measuring the *relative* mental efficiency of groups of children, and secondly, that, assuming that all the tests in the same set are equal in difficulty, the tests have shown their value as a means of measuring "the mental growth and improvement of the various types of subjects, feebleminded, epileptic, psychotic, backward, normal and supernormal."

C. Elizabeth Dealey.

SENESCENCE: THE LAST HALF OF LIFE. By G. Stanley Hall, Ph.D., LL.D. New York and London. D. Appleton & Co., 1922. Pp. xxvii + 518.

President Hall's fond hope of writing the logical complement of his famous "Adolescence" has been achieved. These three volumes taken with "Educational Problems" amply qualify him as the leading constructive "educator" of his day. His "Adolescence" seems to say "Grow old along with me," and his "Senescence," "The best is yet to be, the last of life for which the first was made." Together they constitute a treatise not only of surpassing human interest, but one of basal biological importance in the sociologic range. We miss only an index!

In the make-up of the book first comes a foreword of about a page, and then an introduction of fifteen pages, which serves to introduce the mind of the author even more successfully than it introduces the book—the first adequate contribution in English to "the long-desired but long-delayed science of gerontology." Senescence begins at forty and merges into senectitude or real old age. "What the ignorant, the learned, what the child, the adult and the old man, what science and religion, the savage and the civilized, the Pagan, the Christian, the ancient and the modern worlds have thought on the problems of old age and death he shows, together with his own conclusions upon the questions involved." Stanley Hall should be very well content with his fortune that allows him thus and with other books to round out his heart's desire.

The range and the manner of this work perhaps may best be gathered from a recital of the headings of the long chapters: The first is entitled "The Youth of Old Age," and discusses the change of life in both sexes, Osler's overnoticed wisdom, and numerous other points for this view of life. Chapter II, "The History of Old Age," relates much that is of interest, beginning with age in plants and in brutes, human old age down the ages, and a wealth of men's discussions of the theme from Abraham down to Hrdlicka—sixty-eight pages altogether, full of fact and lore, the starting point, doubtless, of many a future study.

The third chapter, "Literature By and On the Aged," is what its title indicates, as also is the fourth, "Statistics of Old Age and Its Care." Chapters fifth and sixth, respectively entitled "Medical Views and Treatment of Old Age" and "Biology and Physiology," 125 pages together, are about what one would expect them to be. President Hall reveals that he is by no means immune to a now somewhat decadent(?) infection by the closing sentence of these two chapters: "The physiological dominance of sex glands and their products, and the immense rôle played by sex life, especially in man, suggest that it is in this field that the cure of his most grievous ills must be sought, just as the oldest and most persistent myths and legends have so long taught that it was in this field that the so-called fall of man took place"—which seems an assumption that it is still too early radically to confute and sterilize.

Chapter VII is a "Report on Questionnaire Returns." A few score eminent and distinguished old people, mostly "old Americans," were asked

a set of questions, apparently fourteen in number, and the answers are discussed, but none too fully or frankly—a chapter of much human interest but inconclusive, as was inevitable. "Some Conclusions" of the author, however, constitute chapter eight, seventy-three pages long. "Growing old hygienically is like walking over a bridge that becomes ever narrower so that there is progressively less range between the *licet* and the *non licet*, excess and defect. The bridge slowly tapers to a log, then to a tight-rope, and finally to a thread. But we must go on till it breaks or we lose balance. Some keep a level head and go further than others, but all will go down sooner or later." The wisdom of the psychologist is obvious in the important assertion that the *memento mori* brings easement and safeguards us from a dangerous thanatophobia.

The book's last chapter deals with "The Psychology of Death," eighty-one pages, and to the average reader surely is the most interesting part of the book, because the problem of immortality remains the dominant query of the intelligent mind, however strenuously this statement may be denied by some, notably our present sapient author. The present reviewer has the best of authority for believing that Stanley Hall is an agnostic on the question of survival after death, not by any means denying it. Yet the text of this chapter certainly gives a pessimistic impression, verging on denial by implication, to its readers. In fact, the author offers varied ingenious apology for the "fallacious" hope-wish for immortality, and expresses the belief that "heavens, hells, gods, and discarnate ghosts will all fade like dream fabrics or shadows before the rising sun" of man's "true and fully entelechized self." And yet—our human author, wiser than he cares to admit, it seems, ends up his fine treatise with two little lines that lift the impending curse—no less than these:

> "I hope to see my Pilot face to face
> When I have crost the bar."

If the reader has paid his money (and even if he has borrowed the volume from a free library), he may take his choice, as man forever, it may be, will take it.

George Van Ness Dearborn.

U. S. P. H. S. R., New York.

The Dance of Life. By Havelock Ellis. Boston. Houghton Mifflin Company, 1923. Pp. xv + 377.

Planned many years ago, this volume by the famous creator of one of the important treatises of our time, "The Psychology of Sex," gives us a philosophy of life that reminds one of Ruskin, but mostly by contrast-association; there is more substance here. The art of living is set forth in a way that is novel and under the metonymy of a joyful dance. "We verge on philosophy. The whole of this book is on the threshold of philosophy. I hasten to add that it remains there"—which is characteristic of the universal modesty of this great constructive thinker and clear writer. This philosophy sees the world of man as essentially a dance, "a single

metrical stanza in a poem which will be forever hidden from us," a classico-mathematical neo-renaissance, with romantic elements by no means left out. This "Dance of Life" truly is a book as book is defined by its writer: "a revelation of something that had remained latent, unconscious, perhaps even more or less intentionally repressed, within the writer's own soul, which is, ultimately, the soul of mankind." "It is thyself"!

"Veux-tu découvrir le monde,
Ferme tes yeux, Rosemonde.

And here, behold! a new view of it is revealed.

Besides a truly significant Introduction, orientative, there are five chapters and a Conclusion. The chapter titles discuss, respectively, the arts of dancing, of thinking, of writing, of religion, and of morals. The Conclusion sets forth some of the means and motives and successes by which life as an art is quantitative and measurable, and yet, above and beyond all measurement, reaching its apotheosis in some rational and all-accepted system of eugenical euthenics, as the genius of Francis Galton first bade us see. "No creature on the earth has so tortured himself as Man, and none has raised a more exultant Alleluia." "But men still bow down before the fetish of mere quantity in population, and that worship may be their undoing. Giant social organisms, like the giant animal species of early times, may be destined to disappear suddenly when they have attained their extreme expansion."

"The Dance of Life" does not lend itself readily to brief exposition, notwithstanding that exposition would be also interpretation. On the whole, probably, the first chapter proper on the art of dancing itself is the most essential part of the book. One would look far, at any rate, for a better essay on the philosophy of the dance as distinct from its history, biology, and psychology. Few even nowadays (when we are experiencing the relative senescence of almost a dancing mania) realize the fundamental, mighty significance of the dance, or how intimately and universally it is woven into the fabric of our corporeal and social human nature. Is not dancing personified music?

"Dancing as an art, we may be sure, cannot die out, but will always be undergoing a rebirth. Not merely as an art, but also as a social custom, it perpetually emerges afresh from the soul of the people. . . . Dancing has forever been in existence as a spontaneous custom, a social discipline. Thus it is, finally, that dancing meets us, not only as love, as religion, as art, but also as morals. . . . The symphony is but a development of a dance suite, in the first place folk-dances, such as Bach and Händel composed. Indeed, a dance still lingers always at the heart of music and even the heart of the composer. . . . It is, however, the dance itself, apart from the work and apart from the other arts, which, in the opinion of many to-day, has had a decisive influence in socializing, that is to say, in moralizing, the human species. . . . It can scarcely be said that these functions of dancing are yet generally realized and embodied afresh in education. . . . Dancing is the loftiest, the most moving, the most beautiful

of the arts, because it is no mere translation or abstraction from life; *it is life itself. It is the only art,* as Rahel Varnhagen said, *of which we ourselves are the stuff.*" Thus does Havelock Ellis state in still novel terms the proposition, slowly seeping its way downward into the subconsciousness of mankind everywhere, that all music and all joy properly speaking are dancing, either restrained or manifest. And in this philosophy Life itself gets a new interpretation and body a new and important sanction, wider even than it has had before.

To apply this new integration to thought, to writing, to religion, and to morals in a figurative sense is to write a Dance of Life. In this case behold an interesting masterpiece of philosophic essaying, one of the outstanding works of a *savant* whose opinion, because founded as deeply in real science as it is in culture, will attract the ear of the world—and its homage.

GEORGE VAN NESS DEARBORN.

U. S. P. H. S. R., New York.

DREAMS. By H. Tasman Lovell, M.A., Ph.D., Associate Professor of Psychology, The University of Sydney. Sydney, N. S. W. The Australasian Association of Psychology and Philosophy, 1923. Pp. 74. Price 2*s.* 6*d.*

After a preliminary critical sketch of the different attitudes people take toward dreams, Professor Lovell describes the main features of Freud's theory of dreams, and of psychoanalysis, in a manner calculated "to enlist the sympathy of the general public". While this is accomplished in large part by carefully chosen quotations from Freud's own writings, illustrations are taken from Rivers and other sources, and interesting hints at criticism are not lacking. The second part of the monograph is devoted entirely to criticism. (This part, too, is evidently intended to be intelligible to the general reader; but the general reader might have to look up a few terms like cortex, cerebration, thalamus, and protopathic sensibility.) Here the author maintains that "the conception of the 'wish'," and especially the merely libidinous wish, "is not wide enough to cover all the factors that determine dreams". Dreams, and the phenomena of condensation, displacement, dramatization, secondary elaboration, and symbolism, are rather to be traced to some combination of fragments of the waking personality with temporarily uninhibited disparate response tendencies, many of them very primitive. Repression and censorship similarly reduce to inhibition; "while 'dissociation' seems to be an association of antagonism between two elements such that whenever the one tends to rise in consciousness it finds its antagonist already in occupation." Yet, curiously enough in view of this definition of dissociation, the author speaks of tendencies that have been simply "left out" of the synthesis of waking activities, "crowded out for lack of room"; and these tendencies, along with "all that indefinable mass of racial experience of which our remarkable organism is the recorder and repository", constitute for him as for Jung

(in a non-mystical way, Professor Lovell insists) a racial or "collective" unconscious, common to all individuals.

Thus far does the author get in his modestly stated effort "to reconcile traditional psychology with what is apparently incontrovertible in the invaluable system evolved by Freud." And so far as he really reconciles (by selecting and reinterpreting), his effort seems interesting and useful.

W. S. Taylor.

Constructive Conscious Control of the Individual. By F. Matthias Alexander. With an Introduction by Professor John Dewey. New York. E. P. Dutton & Co., 1923. Pp. xxxiii + 317. Price $3.00.

The jacket of this volume has two sentences that save us time and do justice to its author—so why not quote them: "He deals with Sensory Appreciation in its four aspects:—in relation to man's evolutionary development; in relation to learning and learning to do; in relation to man's needs; and in relation to happiness. Mr. Alexander, in fact, has given us here what is practically the second volume of 'Man's Supreme Inheritance' and has stated his basic and original principles of re-education and prevention which may be fully worked out into a system of conscious control of the human self."

As at least a dozen of the present reviewer's articles and books show, some of his own constructive work has attempted to develop this same leading idea of Mr. Alexander's:—ever better and ever adapted appreciation of the sense-fabric as a basis, through kinesthesia, of motor efficiency and act there through of personality-evolution. It almost progresses without conversation, therefore, that the present really humble reviewer looks with surpassing favor on these two books of Alexander's and deems them strivings toward the absolutely basal philosophy of our trial-lifetime. The manner of the volume is a bit cumbersome, perhaps, but the object of the philosophic construction seems ideal:—let every normal child grow up from infancy fully aware of his mechanism of efficiency and thus in manhood fully master of himself in every way. In formulary terms, universal conscious control. This is applied stoicism on a scientific instead of a heroic basis, and yet truly heroic just the same. Education has no ideal higher. As the truly wise introducer, Professor Dewey, says, its "chief difficulty lies in the fact that it is so badly needed," true equally of every forth-reaching phase of world education.

As already has been noted, the treatise is divided into four parts, besides the "Psycho-Physical Attitude," three pages in concluding. Part two ("Sensory Appreciation in Its Relation to Learning to Do") has its scope indicated by its seven chapter titles: "Education and Re-education"; "Incorrect Conception"; "Imperfect Sensory Appreciation"; "Illustration"; "Respiratory Mechanisms"; "Unduly Excited Fear-reflexes, Uncontrolled Emotions, Fixed Prejudices"; and "Psychophysical Equilibrium." There are things in this section of the work that are basally

important to mental hygiene and to all phases of psychotherapeutics save perhaps the most esoteric and Freudistic.

Part three has five chapters: "Knowing Oneself," "Imitation," "Concentration," "Memory and Feeling," and "Complexity and Complications in Relation to Stress and Strain."

Part four is an essay of two dozen pages on happiness in relation to sensory appreciation. It is an apology (in the classic sense) for work and for other forms of necessarily conscious psychophysical activity, continuing one of the noblest things taught to the world anew by the great war with Germany. Continual, insists Mr. Alexander, is the struggle of personality against automaticity with its inevitable submergence into subconsciousness.

Industry, physical education, general education, personality, all need this gospel as they need few things else. Consider, for example, the prehistoric methods still in vogue in manual training—they are as crude still as they were of necessity before the discovery of the sensory nerves and before our knowledge of the mechanism of efficiency had been obtained; not one word, scarcely, of conscious control on the only rational basis of an understanding of the organic mechanism in use! And so, in varying degree, all the way through human activity. Physiology and physiologic psychology are new sciences, to be sure, but decidedly are they not so new that methods of efficiency should have still in 1924 the darkness and ignorance of the Middle Age. This book and its forerunner, "Man's Supreme Inheritance," will do a little to remedy this vast incompetency of method in all kinds of education in the broadest sense, and it will do much more to point out the straight but ever widening way to *real education*, which, in a single word, is personalization, awareness of one's whole self.

It is time that man, "the tool-making animal," should begin to learn how to use intelligently (and therefore, of necessity, consciously) that master mechanism of Evolution, his own motor organism. Yes, this time is long overdue.

George Van Ness Dearborn.

U. S. P. H. S. R., New York.

Statistical Method. By Truman L. Kelley, Professor of Education in Stanford University. New York. The Macmillan Company, 1923. Pp. xi + 390. Price $4.00.

Objective records of observed facts have been the basis on which each of the sciences has been developed. An infinite number of facts about a given subject might be recorded, however, without in any degree developing a real science. It is only when the facts observed have been organized according to their relationships and meanings that a science is finally revealed. Dr. Kelley has in *Statistical Method* prepared a textbook which might well be advertised as "the science of developing a science" or as "methods of interpreting facts." No person who is attempting to make a "contribution to the sum of human knowledge" can well afford to overlook this new textbook, which is much more than a mere summary and

restatement of statistical methods. New formulas, new tables, and new methods are presented in practically every section of the field, and their presentation is both detailed and convincing.

The first chapters of the volume serve to induct one into their subjects much more thoroughly than is usual: I The Tabulation and Plotting of Series; II Graphic Methods; III The Measurement of Central Tendencies; IV Measures of Dispersion; V The Normal Probability Distribution; and VIII Measures of Relationship.

The later chapters provide advanced statistical methods and discussions, which seem at present to be beyond the usual needs of psychologists, educators, physicians, and social workers; but which will doubtless become in a few years the familiar tools of scientific leaders in these fields; VI Comparable Measures; VII The Fitting of Curves to Distributions; IX Functions Involving Correlated Measures; X Further Methods of Measuring Relationship; XI Multiple Correlation; and XIII Index Numbers.

It is not possible to claim that an ordinary high school child can read this volume with pleasure, although the language is remarkably clear and relatively simple. Perhaps the widest use for the text will for the present be as a reference book on the work tables of scientific students. As larger and larger numbers of these students become acquainted with the valuable materials it contains, the book will undoubtedly be used more and more widely as a text for advanced classes in statistical method. The demand for the book will probably grow larger each year for many years to come. America is to be congratulated upon having produced Dr. Kelley and an authoritative textbook in statistics.

M. R. Trabue.

Director, Bureau of Educational Research, School of Education, University of North Carolina.

Behaviorism and Psychology. By A. Roback, Ph.D. Cambridge. University Bookstore, 1923. Pp. 284.

This book is a fervent polemic against behaviorism, and is, as are most other hot polemics, very entertaining reading for those who are in fundamental agreement with the writer but very irritating reading for those who sympathize with the enemy. Such sympathizers are certain to consider the arguments of this book unfair and personal and its style flippant, whereas those who belong to the writer's own camp are likely to take a more or less wicked pleasure in his onslaught, even though their better judgment may approve rather the things that make for peace. Roback has given offense by calling Watson the *enfant terrible* of behaviorism. But this label is not a term of dire reproach. It simply denominates the child who, to the embarrassment of its discreet friends and relatives, says aloud what no one else dares to say. In this sense, Roback himself undoubtedly deserves to be called the *enfant terrible* of introspectionism.

The present writer has lagged with this review chiefly from want of moral courage to enter the battlefield. It is now impossible to write as if

the unfavorabe reviews of Hunter (in the *American Journal of Psychology* for last July) and of Leaming (in the *Psychological Bulletin* for last August) had not appeared. Hence, after a short account of the contents of the book for such readers of this JOURNAL as may not yet have examined it, the reviewer, feeling that Roback has now in his turn been roughly handled, will take heart of grace and pick up the cudgels in his defense.

Apart from its appendices and excellent bibliography, the book falls into four parts. The first part is devoted to putting behaviorism into its historical setting and to pointing out its many varieties, the second to the direct attack upon behaviorism in theory, the third to showing the incongruity between behaviorism and ethics, sociology, medicine, and so on, and the fourth to forecasting the future of behaviorism and of psychology.

The purpose of the book is avowedly (p. 191) "the confutation of the claim of behaviorism to the title of psychology." Behaviorism is that quasi-psychology (this term is not Roback's) which "*reduces psychology to a study of movements* of limb and muscle or gland, more particularly of the body as a whole" (p. 23). The study of human and animal movements is a genuine science which overlaps both physiology and psychology and to which Roback would give the name of *behavioristics*. But behaviorism as a *psychologia militans* is an illiberal and destructive revolt against the introspective method. Though in the main an American product, it harks back in its anti-introspectionism to Comte and in its mechanistic animus to Hobbes.

The soil was prepared for it by the animal objectivists and the refusal of biologists to allow room for consciousness among the topics of science, by the stress which James and others laid upon motor phenomena in connection with consciousness, and alike by neo-realism and by pragmatism in philosophy.

Behaviorism has no standard of orthodoxy. There are behaviorists to whom consciousness "is not only a myth but a profanity," and behaviorists who patronize the study of consciousness. There are those who call themselves behaviorists simply because they wish mainly to use the extrospective method (which is open to any psychologist) and there are behavioristic sympathizers who do not call themselves behaviorists. There are mechanistic behaviorists and there are recruits from the pragmatist camp who—so far as they can be understood—are not mechanistic. As regards that which does the behaving (cf. p. 80 with p. 48 and p. 54), Max Meyer is a nerve-cell behaviorist, Watson is in the main a muscle-and-gland behaviorist, and Holt is a body-as-a-whole behaviorist. Beyond the penumbra of behaviorism the term "behavior" has been so alluring that we find behavior attributed by extreme introspectionists to the phenomena of consciousness itself, and finally we meet with the claim of Calkins that for the psychologist the most important behavior is the behavior of the self. The first psychologist to define his science behavioristically was William McDougall, who forthwith proceeded to contribute not a little to the study of consciousness. In short, such are the divergences and anomalies of behaviorism

within itself that one hardly knows where to hit. In a chart at the end of the book Roback elaborately and perhaps somewhat fancifully classifies all psychologists who can possibly be called behaviorists. (He is not to be held responsible for any wording out of quotation marks in this review.)

The second part of the book will be neglected for the moment. In Part III the writer shows that behaviorism fails to fit in with human thought as embodied in art, ethics, jurisprudence, sociology, medicine, and religion, and even as embodied in the study of intelligence. The moralist, for example, is concerned with conscious motives, and the jurist with the *mens rea*. Such sociologists, as are tyros in psychology, may be captivated by the simplicity, objectivity, and dynamic quality which are suggested by the term "behavioristic psychology," but they should realize that the concept of bodily behavior is totally irrelevant to the psychological *group*, and that the behaviorist (in the strict sense) can predict the kind of conduct in which the sociologist is interested only as the man in the street can predict it. This part of the book is particularly well written. One could not do justice to it even in a much longer summary.

The fourth part forecasts an honorable future for behavioristics when it has learned to know its place in the debatable land between psychology and physiology and a fruitful future for mentalistic psychology when it betakes itself ardently to the study of emotion and volition and when it partially forsakes the academic laboratory for the common haunts of men and for the wards of the psychopathic hospitals.

In the opinion of this reviewer the only serious fault with Roback's portrayal of the case against behaviorism is that he has overcrowded his canvas. His array of arguments is disorderly, some of them are not worked out to the point of clarity, the more important do not stand out at the expense of the less important, and the assemblage makes an impression of unwarranted excitement. The weak portion of the book is Part II, which, in spite of a promise given by its title, fails to sum up in any systematic fashion the case against the principles of behaviorism. But the present reviewer is far from agreeing with Leaming that the book is full of statements whose accuracy it would be hard to prove. Leaming should have adduced specimens of this alleged inaccuracy. Unquestionably Roback's manner is the reverse of judicial, but as to the vivacity of his style, that is by no means a cardinal sin in the present reviewer's eyes. Especially interesting passages are pages 57-58, on which he states his objections to attributing "behavior" to the phenomena of consciousness, and the latter part of Chapter XV, where he points out that the whole development of modern psychology has taken place since Comte utterly condemned introspection as a scientific method.

Hunter, in enumerating five main charges which Roback makes against behaviorism, introduces more order into the arraignment than the book really exhibits. Of these charges the first three are found mainly in the second part of the book, the fifth is the purport of the third part, and the fourth is implicit in the last part.

The first charge is that behaviorism asserts its negative thesis—the

futility of introspection and the inaccessibility of consciousness to scientific examination—and offers no proof but only its own extrospective program and glittering promises of the results to be obtained by it. Hunter's reply savors of *ignoratio elenchi*. After very properly reminding us that even Watson does not dogmatize on the nonexistence of consciousness, and admitting that in his own (Hunter's) opinion consciousness may be made the subject of scientific description and analysis, he retorts that such studies of consciousness as have hitherto been made do not bulk large in the history of psychology. Only a small part, he thinks, of the experimental literature need be regarded as really concerned with consciousness.

Roback's second charge is that the behaviorists have no experimentation to show on lines divergent from introspection. Hunter replies by claiming most of the work upon learning and memory, as well, of course, as all the work in animal and in infant psychology, as done upon nonintrospective lines. With regard to the psychology of learning and memory this claim seems a very large one. But both these two charges and Hunter's replies convince the reviewer that the time is ripe, not for polemics, but for some one to rewrite the history of psychology and psychiatry for the last forty-five years with intent to determine just which of the data that fill our manuals could and which could not have been gleaned without the aid of introspection and analogy. Perhaps as Roback may not know what Watson means when he says he does not know what a mental image means, so Hunter may not know what Roback and some of the rest of us mean by introspection. To believe in the validity and fruitfulness of the introspective method is not necessarily to number oneself with those extremists who can write pages about the mental happenings of ten seconds.

According to Hunter's enumeration, Roback's third charge is that the concept of behavior is too vague and "verschwommen" to be the central concept of a well-defined science. Behaviorists do not agree as to just what the *behaver* is or as to just what movements or other bodily processes are to constitute the unit or bit of behavior. To this Hunter replies in substance (the writer is translating into her own language Hunter's statements as she understands them) that behaviorists *like* not to have a well-defined science. While some of us are railing at their indifference to definitions and assumptions, they are busy increasing the sum of all such knowledge as they find interesting. The reviewer understands from this that behaviorists make it their concern to discover facts but *not to think about them*.

At this point, Hunter—perhaps pricked by Roback's disrespectful application of the word *lingo* to the terminology of behaviorism—dilates upon the advantage of substituting for the terms of traditional psychology—laden as they are with suggestions of philosophical controversy—a new and simon-pure set of terms which can lead no one astray. Since we cannot agree about the meaning of *instinct* and *habit*, we might, he thinks, substitute for them such colorless terms as *alpha* behavior and *beta* behavior. But without definition of the terms for which they are substituted, how are we to know in doubtful cases which term to apply? According to

Hunter, there are no doubtful instances, for he says that "agreement can be reached concerning the observable phenomena." Can it always?

Roback's fourth main criticism is that behaviorism in discarding introspection throws away one possible method of gaining information. To this Hunter replies that Roback overlooks the fact that Watson "would not exclude verbal responses and the testimony of the subject from the methods of psychological investigation." To this the reviewer would in turn retort that if the behaviorist means by "the testimony of the subject" what the introspectionist means by it, and yet is willing to harbor it even as an occasional assistant, then indeed *cadit quaestio.* Instead of two warring factions we have merely a collection of psychologists who vary more or less among themselves in their relative predilection for the methods of introspection and external observation. But if the behaviorist means something else by the testimony of the subject, then *to what does the subject testify?* Surely he does not need to testify to his visible movements and he cannot testify to his nervous and glandular processes. If he testifies to *feeling* his insipient speech reactions, then indeed we have an introspective report in the old sense.

Roback's final objection to behaviorism is its incongruity with the social sciences, with medicine, art and religion. Hunter's reply is that each science should pursue its own end without regard to other sciences, which may take its findings or leave them, as they see fit. This is perfectly true as regards the guiding purpose of a particular science. But as regards the conceptions of a *school* of scientists, after all "congruity is the test of truth" and schools whose views clash with a very large part of the fabric of human thought labor under a very heavy burden of proof. Moreover, it seems to the reviewer that behaviorists have not been especially chary of attempting to apply their science to social matters.

The reviewer's last word is this: Roback has carried the war entirely into the enemy's country. He might well have added to his book a constructive defense of introspectionism. Materials are not lacking. But he has aptly called behaviorism "psychological protestantism" and it is to be remembered that after the Protestant Reformation came the Counter-Reformation. One could wish that the new history of psychology suggested a few moments ago might form part of a methodological treatise written by a man of critical insight but of truly catholic temper, who has interests both in normal and in abnormal psychology and who, though not primarily a philosopher, is also not a victim of philosophophobia. Pending such a treatise, those of us who are moderatists by temperament will gladly allow ample rope to all extremists with the express intent that they may hang themselves.

ELEANOR A. MCC. GAMBLE.

Wellesley College.

NOTES

Dr. Frankwood E. Williams was reëlected medical director of the National Committee for Mental Hygiene at the annual meeting of the Board of Directors, held in New York City, on December 28. The following were elected members of the Executive Committee: Dr. William L. Russell, medical director, Bloomingdale Hospital, White Plains, New York; Dr. Walter E. Fernald, superintendent, Massachusetts School for the Feebleminded, Waverley; Dr. Stephen P. Duggan, director, Institute of International Education, New York City; Dr. William A. White, superintendent, St. Elizabeths Hospital, Washington, D. C.; Dr. Charles P. Emerson, dean of the Medical school, University of Indiana, Indianapolis; Dr. C. Floyd Haviland, chairman, State Hospital Commission, Albany, New York; Dr. Arthur H. Ruggles, superintendent, Butler Hospital, Providence, Rhode Island; and Mr. Matthew C. Fleming, attorney, New York City. Dr. William H. Welch, president of the National Committee for Mental Hygiene, presided.

The annual meeting of the Southern Society for Philosophy and Psychology will be held at the Johns Hopkins University on April 22d and 23d. There will be a Symposium on Psychology and Religion on the evening of the 23d, in which Professor Leuba of Bryn Mawr, Professor Lovejoy of Johns Hopkins, and Professor Sanborn of Vanderbilt will participate.

The Psychological Laboratory will be open for inspection on the afternoon of April 21st, the day preceding the meeting.

The American Sociological Society devoted one of its sessions at its last annual meeting at Washington, D. C., to an interesting program on social psychology. By invitation of the society papers were read upon psychological questions fundamental for social science by Professor McDougall, Harvard University; Professor Kantor, Indiana University, and Professor Allport, University of North Carolina. These papers were ably discussed by members of the American Sociological Society. The three papers, together with the discussions, will appear in an early number of the JOURNAL.

NEW BOOKS RECEIVED

(This notice does not preclude the book from further review or comment.)

Nervous and Mental Re-Education. By Shepherd Ivory Franz. New York. The Macmillan Company, 1923. Pp. ix+225. Price $2.00.

General Introduction to Psychology. By Coleman R. Griffith. New York. The Macmillan Company, 1923. Pp. xv+513.

Psychologie et Developpement De L'enfance a La Vieillesse. By Ch. De Montet et H. Hersot. Berne. Ernest Bircher, 1922. Pp. viii+221.

Jose Ingenieros. By Julio Endara. Buenos Aires. Agencia General De Libreria. Pp. 100.

Personality and Social Adjustment. By Ernest R. Groves. New York. Longmans, Green and Company, 1923. Pp. ix+296. Price $1.40.

Personality. Studies in Personal Development. By Harry Collins Spillman. New York. The Gregg Publishing Company, 1923. Pp. 206. Price $1.50.

Handbook for Mental Nurses. Published under the authority of the Medico-Psychological Association. Chicago. Chicago Medical Co., 1923. Pp. xiii+615. Price $2.50.

Studies in Psychoanalysis. By Charles Baudouin. New York. Dodd, Mead and Company, 1922. Pp. xxiv+508.

Sociology and Political Theory. By Harry Elmer Barnes. New York. Alfred A. Knopf, 1924. Pp. xvii+260.

The Truth About Spiritualism. By William S. Sadler. Chicago. A. C. McClurg and Co., 1923. Pp. vii+211.

Adolescent Interests. By F. I. Davenport. Archives of Psychology, No. 66. 1923. New York. Pp. 62.

Human Nature and Conduct. By John Dewey. New York. Henry Holt and Co., 1922. Pp. vii+336.

Social Problems and Social Policy. Edited by James Ford. Boston. Ginn and Company, 1923. Pp. xiii+1027. Price $4.00.

Personnel Management. By Walter Dill Scott and Robert C. Clothier. Chicago. A. W. Shaw Company, 1923. Pp. xii+643.

The Re-creating of the Individual. By Beatrice M. Hinkle. New York. Harcourt, Brace and Company, 1923. Pp. xiii+465.

Intelligence Testing. By Rudolf Pintner. New York. Henry Holt and Company, 1923. Pp. vii+406.

The Control of the Social Mind. By Arland D. Weeks. New York. D. Appleton and Company, 1923. Pp. xvi+263.

The Birth of Psyche. By L. Charles Baudouin. New York. E. P. Dutton & Co., 1923. Pp. xxii+211. Price $2.00.

Studies in Mental Deviation. By S. D. Porteus. Vineland, N. J. Publications of the Training School at Vineland, N. J., No. 24, 1922. Pp. xi+276.

Aberrations of Life. By J. C. McKerrow. New York. Longmans, Green & Co., 1923. Pp. 107. Price $2.00.

An Introduction to Psychology. By Susan B. Brierly. New York. Dodd, Mead & Co., 1923. Pp. vii+152. Price $2.00.

Applied Psychology for Nurses. By Donald A. Laird. Philadelphia. J. B. Lippincott Co., 1923. Pp. ix+236. Price $2.50.

Social Psychology. By Floyd H. Allport. Boston. Houghton Mifflin Co., 1924. Pp. xiv+453. Price $2.50.

Directory of American Psychological Periodicals

American Journal of Psychology — Ithaca, N. Y.: Morrill Hall. Subscription $6.50. 500 pages annually. Edited by E. B. Titchener. Quarterly. General and experimental psychology. Founded 1887.

Pedagogical Seminary — Worcester, Mass.: 950 Main Street. Subscription $5. 575 pages annually. Edited by G. Stanley Hall. Quarterly. Pedagogy and educational psychology. Founded 1891.

Psychological Review — Princeton, N. J.: Psychological Review Company. Subscription $4.25. 480 pages annually. Bi-monthly. General. Founded 1894. Edited by Howard C. Warren.

Psychological Bulletin — Princeton, N. J.: Psychological Review Company. Subscription $5. 720 pages annually. Psychological literature. Monthly. Founded 1904. Edited by Samuel W. Fernberger.

Psychological Monographs — Princeton, N. J.: Psychological Review Company. Subscription $5.50 per vol. 500 pp. Founded 1895. Ed. by Shepherd I. Franz. Published without fixed dates, each issue one or more researches.

Psychological Index — Princeton, N. J.: Psychological Review Company. Subscription $1.50. 200 pp. Founded 1895. Edited by Madison Bentley. An annual bibliography of psychological literature.

Journal of Philosophy — New York; Sub-Station 84. Subscription $4. 728 pages per volume. Founded 1904. Bi-weekly. Edited by F. J. E. Woodbridge and Wendell T. Bush.

Archives of Psychology — Sub-Station 84, N. Y.: Archives of Psychology. Subscription $5. 500 pp. ann. Founded 1906. Ed. by R. S. Woodworth. Published without fixed dates, each number a single experimental study.

Journal of Abnormal Psychology and Social Psychology—Boyd Printing Co., Inc., Albany, N. Y. Subscription $5. Edited by Morton Prince and Floyd H. Allport. Quarterly. 432 pages annually. Founded 1906. Abnormal and social.

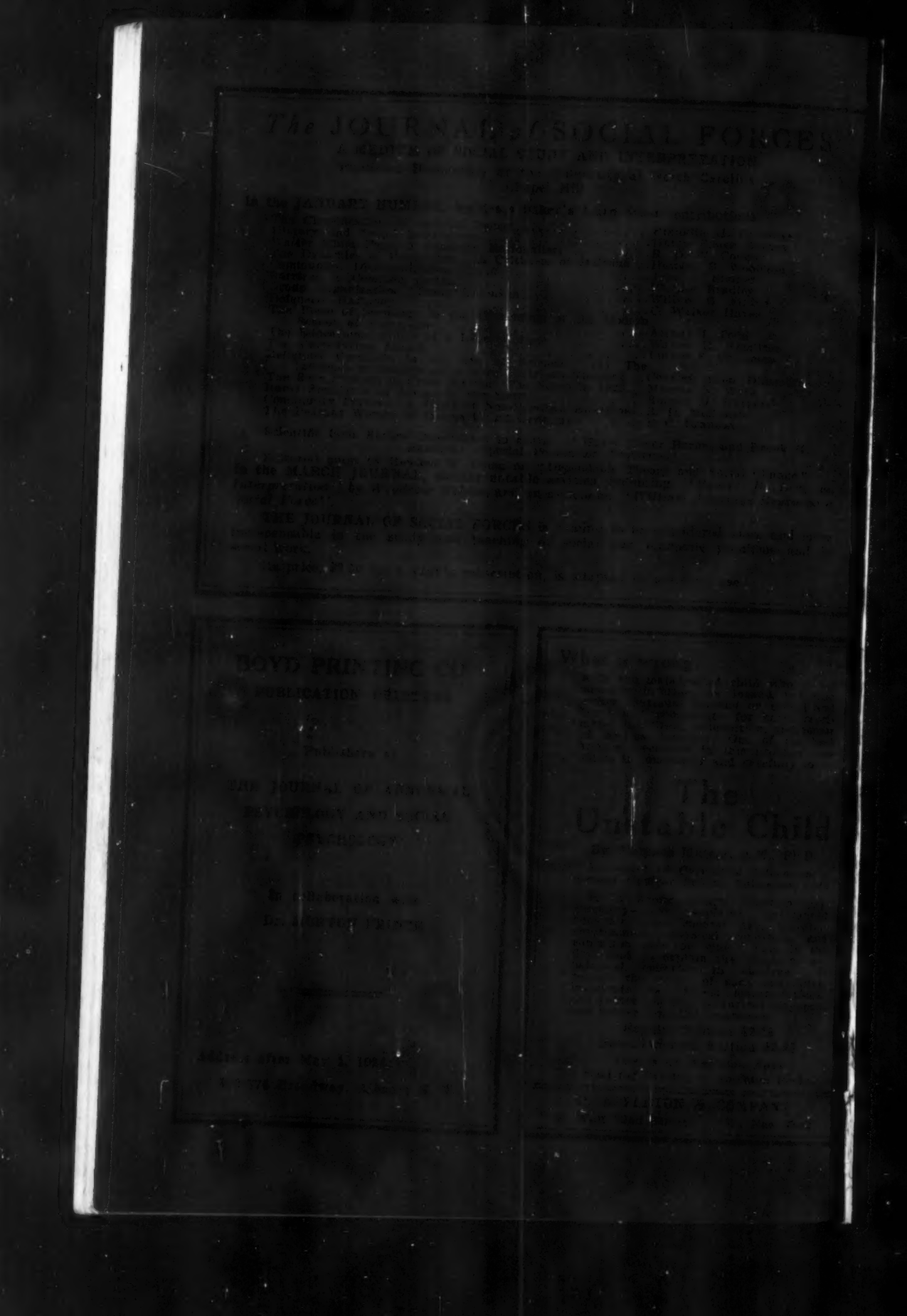
Psychological Clinic — Philadelphia: Psychological Clinic Press. Subscription $2.50. 288 pages. Ed. by Lightner Witmer. Founded 1907. Without fixed dates (9 numbers). Orthogenics, psychology, hygiene.

Training School Bulletin — Vineland, N. J.: The Training School. Subscription $1. 160 pp. ann. Ed. by E. R. Johnstone. Founded 1904. Monthly (10 numbers). Psychology and training of defectives.

Comparative Psychology Monographs — Baltimore: Williams & Wilkins Co. Subscription $5. 500 pages per volume. Edited by W. S. Hunter. Published without fixed dates, each number a single research.

Psychoanalytic Review — Washington, D. C.: 3617 10th St., N. W. Subscription $6. 500 pages annually. Psychoanalysis. Quarterly. Founded 1913. Ed. by W. A. White and S. E. Jelliffe.

Journal of Experimental Psychology — Princeton, N. J.: Psychological Review Company. 480 pages annually. Experimental. Subscription $4.25. Founded 1916. Bi-monthly. Ed. by John B. Watson.

Journal of Applied Psychology — Worcester, Mass. Florence Chandler. Subscription $4. 400 pages annually. Founded 1917. Quarterly. Edited by James P. Porter and William F. Book.

Journal of Comparative Psychology — Baltimore: Williams & Wilkins Co. Subscription $5. 500 pages annually. Founded 1921. Bi-monthly. Edited by Knight Dunlap and Robert M. Yerkes. (P. B.)